American Political Science Review

JUNE 1994
VOLUME 88 NUMBER 2

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The *American Political Science Review* (ISSN-0003-0554) appears quarterly. It is published by the American Political Science Association, 1527 New Hampshire Ave., N.W., Washington, D.C. 20036, and sent to all members. Dues: Regular Members with gross calendar income: under $30,000, $55; $30,000–$40,000, $75; $41,000–$50,000, $85; $51,000–$60,000, $90; over $60,000, $95; Student Members (limited to 5 years), $25; Retired Members (who have been members 25 years) with gross calendar income; under $25,000, $20; $25,000 and over, $40; Life Members, $2,000; Institutional Members: Domestic, $147; Foreign, $165. Dues allocated for a subscription to the *APSR*: $17 for individual members; $110 for institutional members. Changes of address sent to Membership Secretary of the APSA. Postmaster: Send address changes to: APSR, 1527 New Hampshire Ave., N.W., Washington, D.C. 20036. Second-class postage paid at Washington, D.C. and at additional mailing offices. Copyright © 1994 by the *American Political Science Association*.

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QUARTERLY JOURNAL OF THE
AMERICAN POLITICAL SCIENCE ASSOCIATION

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VOLUME 88 NUMBER 2

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Indexing

Articles and notes appearing in the *Review* before the June 1953 issue were indexed in *The Reader's Guide to Periodical Literature.* Current issues are indexed by *International Political Science Abstracts, United States Political Science Documents,* and *Social Sciences Index.* Microfilm of the *Review,* beginning with Volume 1, may be obtained from University Microfilms, 300 North Zeeb Road, Ann Arbor, Michigan 48106. A *Cumulative Index of the Review,* Volumes 1–62, 1906–1968, also may be obtained from University Microfilms. Articles appearing in the *Review* are listed in *ABC Pol Sci* and *Current Contents: Behavioral, Social & Management Sciences.* Book Reviews are indexed in *Book Review Index.*

ARTICLES

American Political Science Review Vol. 88, No. 2 June 1994

CHOOSING JUSTICE: SOCRATES' MODEL CITY AND THE PRACTICE OF DIALECTIC

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Glaucon's demand to be shown the inherent choiceworthiness of justice exposes the limits of dialectical argument. Acknowledging these limits, Socrates proposes that his interlocutors join him in an alternative activity, making a city in speech. This model city proves to be "entirely opposite" to existing cities, above all (as Socrates observes) because it restricts the practice of dialectical argument to those who first demonstrate a capacity for synopsis, that is, for seeing things as a whole. Socrates holds that one must be able to see things as a whole in order to benefit from the use of dialectic. I interpret the political institutions of Socrates' model city accordingly, as being instrumental to the practice of dialectic. Hence, I reject the prevalent readings of the Republic, *which present these institutions either as a blueprint for public policy or as a parody of political idealism. Instead, I suggest that the interlocutors' discussion of censorship, the noble lie, and communism is propaedeutic, fostering the synoptic capacity necessary to benefit from the practice of dialectic, including dialectic aimed at revealing the choiceworthiness of justice.*

Midway through his conversation with Thrasymachus in book 1 of Plato's *Republic,* Socrates is persuaded to postpone his quest for a precise definition of justice, for "what Thrasymachus is saying now seems to me to be much more important" (347e2–3).[1] Thrasymachus, we recall, had abandoned his original thesis—that justice consists in the advantage of the stronger—and has taken up a new position, contending that the unjust life is stronger and better than the just life. This shift prompts Socrates to turn to his young companion, Glaucon, and pose two questions: "Which way, Glaucon, will you choose? And which seems argued the more truly to you" (347e4–6)? The second question is by no means superfluous.[2] Socrates evidently anticipates that Glaucon's preference may rest upon something other than the better argument. Indeed, it is not so much clear thinking as *native breeding* that Socrates credits when he praises Glaucon's inclination toward justice (*tên physin . . . tropou,* 367e6–368b2). Quality of breeding is an admirable thing, of course; but it is hardly to be mistaken for mature human excellence. Nor is Glaucon's *tropos*—his present "bent" or inclination—indicative of a firm or decisive choice for justice. On the contrary, Glaucon remains fascinated by the tyrannical, or perfectly unjust, life. Moreover, he supposes that to possess mature human excellence (whether that be justice, injustice, or the clever alternation of both), one must also be able to give a rational account confirming its superiority. So although Glaucon is inclined to prefer justice, he feels he has lost his way because he has not yet heard a satisfying proof of its superiority (358c6–8).

The precariousness of Glaucon's circumstance is only aggravated by the extraordinary quality of his heart and mind. The radiance of his intellect renders Thrasymachus and other likeminded lights of the sophistic firmament virtually invisible as Glaucon introduces an eidetic or formal structure to the interlocutors' rambling consideration of justice. Justice, he suggests, is intelligible as a tertiary kind of good, desired not for its own sake but only for its consequences—consequences that might be acquired even more efficiently by other means. Moreover, as Socrates testifies, Glaucon is "always and in everything superlatively courageous" (*aei te dê andreiotatos . . . pros hapanta,* 357a2–3). This superlative courage or manliness (the Greek permits either translation) is perhaps most evident in Glaucon's taking up the cause of injustice. For he thereby puts his own admirable nature in jeopardy in hope of acquiring mature human excellence. Still, it is one thing to recognize such courage as a prerequisite in the quest for mature human excellence and quite another to tout it as a sufficient criterion by which to decide whether or not justice counts as that excellence.[3] Yet Glaucon declares (putatively for the sake of argument) that he will henceforth accept justice as human excellence only if it can be shown to profit a "real man" (*hôs alêthôs andra,* 359b2). The real man, as Glaucon conceives him, is not shackled by the bonds of necessity that tie others to the civic community. It is inferior human beings who are compelled to seek justice as an alternative to what they most fear (suffering injustice) and what they naturally desire (committing injustice). Justice, thus understood, is merely a convenient mediocrity, not an excellence of soul. Glaucon thus foresees the distinctive gambit of all modern political thought by identifying the root of justice in a utilitarian compromise or social contract. But his proud spirit will not abide the egalitarianism presupposed in the typically modern development of this gambit. On the contrary, he recognizes that the social contract theory of justice, which looks to the inferior rather than to the superior specimen, demonstrates only that justice is circumstantially choiceworthy, not that it is in any way essentially so. Glaucon accordingly challenges Socrates to praise justice in itself. Socrates must show the power of justice when it is present all by itself in the soul, apart from its wages and consequences (358b4–7, d1–2). Nay, Socrates must reverse the reputations of the just man and the unjust man to ensure that the judgment of their lives will be rendered correctly and not settled for the sake of consequences alone (360e1–6, 361b5–d3).

Glaucon claims to be unimpressed by conventional teachings in favor of justice. So besides providing the indispensable root of extraordinary courage, Glaucon's manly spirit evidently encourages an exaggerated sense of his own freedom from mere conventionality. He needs reminding (as Socrates points out) that the prisoners in the cave of political culture are "like us" (515a5). In view of Glaucon's exaggerated sense of independence, we may expect that beneath the veneer of his bold challenge to conventional justice lies a core of unexamined conventionality. Glaucon, we may say, does not sufficiently know himself. Adeimantus, his fearless yet less enterprising brother, is more perceptive than Glaucon in this regard. Undaunted by his brother's dazzling account of the superiority of injustice, Adeimantus coolly notes that Glaucon has failed to identify "the very thing that most needs to be said" (362d5). Adeimantus elaborates this most needful consideration, which will in good measure determine the course of the interlocutors' subsequent conversation. In so doing, Adeimantus observes that Glaucon's challenge to Socrates fails to take into account the influence of customary teachings concerning virtue and vice on the souls of the youths who hear them (365a6–b1). As a result, Adeimantus suggests, Glaucon fails to realize that even he is not yet in a position to make a meaningful choice between justice and injustice—no matter what Socrates says by way of response to his challenge.

According to Adeimantus, the customary teachings concerning virtue and vice subvert the soul's capacity for acquiring mature human excellence. Due to a distortion of soul induced by such teachings, we too may be ill equipped to make the choice that Glaucon longs to make and that we too must make if we are to move beyond dispositional inclination toward mature excellence. It is with this counterproductive education in view that Adeimantus bids Socrates "not only to show us by rational argument that justice is mightier than injustice, but also to show what it is that each by itself makes [*poiousa*] of the one who possesses it" (367b2–5, e1–5). Though less dazzling than his brother's challenge, Adeimantus' charge is in fact more radical. For Glaucon's challenge merely calls for a static account of justice's superiority, an account revealing justice as pleasing to the human soul as currently disposed. But Adeimantus correctly discerns that Glaucon's desire to make an unprejudiced and fully informed choice between justice and injustice requires a dynamic account, an account recognizing that the choiceworthiness of justice depends upon its unique capacity for healing the deformity from which the conventionally educated soul currently suffers.

SYNOPSIS AS A CURE FOR PSYCHIC DEFORMITY

What, then, according to Adeimantus, is the conventionally induced deformity of soul that stands in need of healing? Adeimantus begins to elaborate the nature of this deformity by noting the counterproductivity of the arguments customarily offered to encourage justice. Parents and poets (who are so often the architects of civilization) conspire in citing for the edification of youngsters the most tangible and appealing consequences that can possibly be ascribed to justice. As it turns out, however, such tangibly appealing consequences do not belong exclusively (or even necessarily) to justice. So the speeches made in support of justice with the best of intentions only inflame a callow desire for things that are, if anything, more easily obtainable by foul means than by fair. When youngsters raised in this fashion encounter arguments of a different cast contending that injustice is shameful only by opinion and that the gods themselves are willing to overlook one's crimes in exchange for ceremonial bribes, they realize, Adeimantus says, that "one ought to turn wholly toward appearances" (363e5–365c3). It is the exclusivity or one-sidedness of this "turn" that deforms the soul. For a soul turned *wholly* toward appearances is indeed like a captive compelled to gaze at shadows cast upon the wall of a cave. The human soul in such a condition is deprived of its capacity for reflection upon the linkage, be it ever so tenuous, between shadow and substance. When appearance becomes everything, appearances no longer mean anything. Once freed of any mooring in reality, appearance no longer offers even an oblique clue to what truly is. Appearance then exists as nothing more than an invitation to the projection of fantasy and a temptation to willful manipulation. As a result of the soul's turn wholly toward appearances, reason becomes cramped and deformed. Reason ceases its search for the true and the good in considering the merits of justice and injustice; instead, it is wholly subsumed in "calculation" (*logizomenos*), specifically in that calculation which concludes that injustice is mightier than justice always and everywhere as long as one has the capacity to manipulate appearances (366a6–7). From this perspective the comparison of just and unjust lives becomes a forgone conclusion rather than the preparation for a decisive choice. Thus Adeimantus perceives that one's soul must be healed—its whole orientation toward appearances corrected—before one is prepared to make a judgment regarding human excellence. He perceives, moreover, that it is unlikely that *any further argument* could provide such healing (366b3).

It is, of course, paradoxical to identify Glaucon as a victim of the conventional, soul-deforming process of childrearing that is described by Adeimantus, for it seems that much of what we find appealing in Glaucon derives from his unyielding rejection of convention. But it would be premature, to say the least, to describe Glaucon's scorn for social bonds as sufficient to effect the genuine turnabout, or *periagôgê*, that Socrates later identifies as essential to the soul's pursuit of wisdom (518c4–d1, 520c5–8). Yet Glaucon hardly seems guilty of the fixation upon appearances condemned by Adeimantus. If anything, Glaucon may be said to hold appearances (i.e., what seems to be, as distinguished from what truly is) in utter contempt (357a4–b4, 361c4–d3). But one wonders whether the scorn and contempt that one observes in Glaucon's rejection of conventionality is a mark of liberation or is itself an advanced symptom of the psychic distortion Adeimantus has described. Perhaps Glaucon's demand to hear justice praised "in itself and for itself" (*auto kath' hauto*, 358d2) is merely the inversion typical of psychological denial, in which he transposes the theme Adeimantus finds characteristic of traditional childrearing. Consideration of this possibility might remain purely speculative were it not for the fact that Socrates, too, indicates the inadequacy of Glaucon's contemptuous turn from appearances. For Socrates identifies as "the most beautiful or noblest" (*kallistôi*) not the kind of good that Glaucon describes as desirable "in itself and for itself" (*auto hautou heneka*, 357b6) but the kind that is to be loved "both on account of itself and on account of the things coming into being from it" (*kai di' auto kai dia ta gignomena ap' auto*, 358a1–2). Socrates thereby suggests that Glaucon's turn toward the thing itself might well be an impediment to the discovery of the finest, noblest, and most beautiful things, whose discernment requires a *comprehensive* glimpse of the thing itself *and* what comes into being from it. Socrates thus indicates that if Glaucon is accurately to determine the character of human excellence, he must first develop the capacity for *synopsis*, or comprehensive vision. This is the same capacity that Socrates later explains is necessary if one is to benefit from dialectical argument (537b8–539d7). Moreover, Socrates describes Glaucon's ideals of perfect justice and injustice as "statues," that is, mere images (360e1–361d6). Socrates thus suggests that by heaping contempt upon appearances in his probing for reality, Glaucon will never get beyond the shadowy realm of images. The reality he supposes he has discovered turns out to be just another figment upon the wall of the cave. On this basis, I hazard to say, we may conclude that Adeimantus' diagnosis indeed applies to Glaucon. Glaucon's contempt for appearances does not correct but simply inverts the warp from which the human soul suffers in the cave of political culture. Although this warp is endemic to the political cave, the case of Glaucon reveals that neither is its cure to be found in the mere rejection of political culture. The soul must receive political culture (i.e., the culture of *politeia*) but not in the manner of a cavedweller. Is there any way for this to occur?

Although Socrates says that he "had something in mind to say" in answer to Glaucon's challenge, the addition of Adeimantus' speech lands Socrates in a state of genuine perplexity or aporia (cf. 362d1–2 and 367e6–368b4). Socrates realizes that a *logos* accounting for the superiority of justice is needed and yet that no further argument can benefit Glaucon as long as he remains in his present condition of synoptic incapacity. If anything, the operation of dialectical argument will inevitably deteriorate into eristics (as happened between Socrates and Thrasymachus) if the distinctions one makes fail to correspond to the proper articulations of the whole (454a1–9). If we are not merely to quarrel but are really "to get things sorted out" (*dialegesthai*), we must not lose sight of the forest for the trees. Yet this is precisely what happens to Glaucon. His contemptuous turning against political convention (and against appearance generally) resembles nothing else more than "the spinning of a shard" (521c5). The shard, or *ostrakon*, a fragment of smashed pottery, perfectly symbolizes Glaucon's condition. For his contemptuous disengagement from political culture leaves him an isolated piece of a shattered whole. As a shard, Glaucon cannot adequately contemplate the human good. Glaucon must develop a capacity for synopsis (appreciating himself and others as members of a greater whole) if he is to heal his own soul and to attain a position from which he can accurately discern the nature of human excellence. But if dialectic is ultimately needed to reveal the truth about justice and injustice and yet would degenerate into eristics if practiced at present, how is Socrates to proceed? The salvation of Glaucon's soul—the preservation and enhancement of his capacity for human excellence—evidently depends upon Socrates' success in a propaedeutic effort that differs from and yet must prepare for the actual practice of dialectic.

Socrates' response to the challenge posed by the speeches of Glaucon and Adeimantus is to postpone further attempts at dialectic. Instead, he proposes that his interlocutors join him in the nondialectical—one may even say, poetic (*poiômen*, 369c9)—exercise of making a city in speech. It is hardly coincidental, then, that one finds no form of the word *dialegesthai* in the text again until the opening of book 5, where it is spoken to denote the division of the city's women in accordance with the eidetic structure of the whole political community (see 454a4–d1). The absence of *dialegesthai* from the interlocutors' own conversation in books 2–4 mirrors the postponement of dialectic that Socrates requires of the citizens of his model city, which he eventually credits for making this city "completely opposite" to existing cities (497e5–498c4). The interlocutors' forbearance from dialectic in the course of founding the model city is complemented by their approval of certain controversial political institutions (censorship, the noble lie, and communism) which approval, as I shall suggest, helps to foster their synoptic capacities and thus to prepare for the beneficial practice of dialectic.

POSTPONING AND PROVIDING FOR DIALECTIC: THE SOCRATIC TURN TO POLITICS

Though Socrates' interlocutors want to see for themselves the power of justice in or on the human soul, they evidently do not appreciate sufficiently the difficulty of attaining an adequate view of the soul. To attempt such an inquiry is no slight matter. In Socrates' frank estimation, it requires a keen observer (368c7–8). So it would be futile for one who is "not at all sharp-sighted" (368d3–4) to try to investigate this question immediately. But if such a person, someone who was unable to distinguish certain small letters, were able to find elsewhere the same letters "bigger and in a bigger place," he might well consider it a godsend. Supposing that justice pertains not only to a single man but also to a "whole city" and that the city is bigger than a single man, Socrates suggests that his interlocutors behold a city as it comes into being in speech. For then their inquiry could proceed by "investigating the similarity of the larger in the *idea* of the smaller" (369a1–3).

Socrates' investigative strategy is puzzling. How can this strategy succeed, one wonders, except on the *assumption* that the two sets of letters are in fact the same? And how is this assumption to be verified if the inquirer has taken the trouble to consider the larger letters only because he cannot make out the smaller? This conundrum vitiates any attempt to construe Socrates' turn to politics as a *method* by which even the intellectually myopic can identify justice in the soul. The activity of making a city in speech is not an investigative methodology but rather a *therapeutic exercise*. Socratic politics are a calisthenic, not a crutch. They seek to foster, rather than to make unnecessary, the sharp-sightedness required for the success of subsequent research. If successful, the therapeutic exercise of making a city in speech will make it unnecessary to rely upon unsubstantiated parallels or correspondences. Specifically, the interlocutors' foundation of the model city will fortify their own capacities for synopsis, for seeing things as a whole. This result will, of course, be most evident in Glaucon (as we shall see), because his bold vision is at once the most penetrating and yet the least synoptic of any of Socrates' interlocutors.

As they undertake the exercise of founding a city in speech, the interlocutors agree that justice will be present in a "whole city" (*holês poleôs*, 368e2–3). But the precise requirements of civic wholeness are not immediately clear. The political community, by reason of serving the purposes of human living, necessarily embraces an abundance and multiplicity of human goods. But because we neither know what is the greatest of these goods nor have a basis upon which to choose reliably among the multiplicity of apparent goods, we cannot yet be sure we know when the city has achieved completeness or wholeness (thus *isôs*, 371e11). Being unable to discriminate among goods, one really has no alternative but to welcome them all and then to try to determine in what manner and to what extent they might fit together. By making a city in speech, one is thus encouraged to consider the possibility of order among the multiplicity of human goods. Such an exercise cultivates the capacity for synopsis. So it is, I shall suggest, that the practices of Socrates' conversational community will heal Glaucon's soul, providing what the political culture of no existing city could provide.

Socratic Censorship and the Genesis of Political Order

Socrates and Adeimantus undertake the foundation of the first city, which Socrates describes as truthful (*alêthinê*, 372e7). He means that nothing here is concealed. In other words, there is in this city an unproblematic relationship between what appears and what really is. One reason for this is that the men and women of this first city hardly desire anything beyond the plain necessities that life itself demands. They eat and dress as simply as possible. They are industrious in their work, songful in prayer, and delighted in intercourse but wary of overpopulation, so as to avoid poverty and war (372a5–c1). We may say that in such circumstances, there resides truth and nothing but the truth. But we cannot say that the *whole truth* is present in this city. For Glaucon's peremptory rejection of the complacent lifestyle of its residents rebuts any interpretation that construes the "truthful" city to be wholly true to human nature—at least to human nature as we know it, which includes the very spiritedness epitomized here by Glaucon's reaction. It would be utopian in the most pejorative sense of the term to attempt to construct a city on the basis of the suppression of such spiritedness, or *thymos*. Such a city would indeed be "fit for pigs," rather than for human beings (372d4–5). A political community that is to be *wholly* truthful must somehow comprehend Glaucon as a member.

But what is it that the city must become in order to comprehend Glaucon as a member? The price of including spiritedness, at least in the short run, is the introduction of unnecessary desires. Glaucon desires what he calls "relish" (*opsou*, 372c2) as a supplement to the indispensable staples of human life. Lacking as yet any better understanding of the true relish of life, he insists upon the inclusion of the full array of goods and services commonly accepted as pertaining to an elegant lifestyle. Socrates accedes to Glaucon's demand because "nothing hinders it" (372e8). That is to say, nothing in the character of the city hinders it. Reflecting its founders' lack of wisdom, the city is in no position to exclude any good or service as long as its provision is supervised by some skilled professional. This city knows only one principle: one man, one art (370b4–6).[4] Technical expertise thus holds a monopoly on authority in the first city. There is, consequently, no hierarchy or order among the myriad of consumer goods that the city permits. Like the inflamed appetite it is obliged to admit, the city is itself limitless. It possesses as yet no principle according to which its order or even its own completion may be determined (371e9–11). In short, the attempt to supply Glaucon's demand for relish produces nothing more than a "promiscuous heap, stuffed and multitudinous" (373b3–4).

Even Glaucon's final demand—for an expert militia—merely results in the further accretion of specialists recruited for the service of limitless appetite. It may be that the city needs such warriors, but what is there to keep these watchdogs from turning wolf and devouring their fellow citizens? The prospect of civil war, confronting the founders at the culmination of a protracted period of civic expansion, vividly indicates that the city's wholeness is not to be sought in the unrestricted increase of the goods and services that it includes. This wholeness evidently depends instead upon the establishment of a principled relationship among the city's diverse parts. The Greek word for a principled relationship or authoritative order of this sort is *politeia*. Absent a *politeia*, the city remains too miserable a thing (even in the throes of its great crisis) to hold Glaucon's attention; he expresses no interest in the search for a means to defend against the warriors' abuse of power. We may even surmise that Glaucon would find wolfish warriors (who are, at least, real men) more attractive than the sheeplike city. So it is Glaucon's brother, Adeimantus, who dutifully joins Socrates in seeking an education appropriate to civic guardians (376d4).

What is most remarkable about the ensuing discussion of education is its establishment of a *politeia* for the city. A *politeia* emerges as the founders censor the practice of technical expertise in order to ensure that the city's warriors' will become true and virtuous guardians (395c4–d6).[5] Something beyond the founders' exhaustive and exhausting survey of artisans and consumers finally comes into sight in the polity, which arises only with the introduction of censorship. This censorship is conducted in accordance with certain theological *typoi* (outlines or impressions) that are formulated with a view both to the formation of politically hygienic opinions and to the truth. The poetic tales that violate these *typoi* incite politically dangerous self-indulgence and "are not true, either" (378c1). It is the proper business of the city's founders to know these *typoi* even if the task of filling them in must be turned over to poets willing to work within their limits (379a1–4). Socrates and Adeimantus identify four guidelines that are to characterize the tales told by the poets to the city's youth:

1. Divinity is the cause of only that which conspires for the good (380c8–9).
2. Particular divinities do not deceive us by altering their proper forms (*eidei*) and assuming alien ones (380d1–6, 383a3–5).
3. The realm of the divine Hades is not so terrifying as to warrant the choice of a shameful life over a noble death (386a6–c2, 387b5–6).
4. Heroes, if they are truly the progeny of divinities, will not lose their heads in regard to grief, vengeance, levity, dishonesty, drink, sex, food, or money (387d4–392a2).

As Socrates reveals, the practice of censorship does not stem from any independent understanding of an order among the multiplicity of human goods (392a3–11). It derives, instead, from the founders' *subordination* of other claims to these elements (nay, these bare outlines) of theology as they attempt to ensure the goodness of the city's guardians, which they take to be the most reliable safeguard of civic wholeness. Thus, Socrates and Adeimantus hold that even if there were to appear "a man enabled by wisdom to represent all things" (*hypo sophias . . . mimeisthai panta chrêmata*), he should be promptly escorted to the city's frontier and sent away; for such an expert would not "harmonize with our *politeia*" (397d10–398a7). Evidently, *politeia* (mentioned here explicitly for the first time in the dialogue) comes into sight only with the discovery of a principle of organization that outranks technical expertise. Yet surely it is paradoxical that Socrates would banish the very man he credits with wisdom. Granting that the mimetic expert (i.e., the all-wise and unsubordinated poet) would not "fit in," we may still wonder why it is the poet, rather than the *politeia*, that must go. Why, in other words, sacrifice wisdom for the sake of political order? And if the wisdom of expert proficiency is no longer sufficient to warrant citizenship in the model city, what takes its place? What is the nature of this new principle and how does it justify the exclusion of a wise man?

It is not easy to see that the statesman has authority over the poet, much less to identify the foundation of such authority. Yet the existence of such authority would imply that the statesman's recognition of theological guidelines—the only things that the founders as yet claim to understand—outranks the poet's wisdom concerning "all things."[6] But in what sense, if any, is the statesman's knowledge superior to the poet's? As Socrates has formulated it, the comparison would seem to favor the poet. After all, the founders of the model city are said to recognize *outlines*; their knowledge is far from complete. The poet's wisdom, by contrast, touches upon all things. Hence its epic scope. Poetic mimesis thus appears as the natural rival of philosophy. For its reach over the totality of things is easily mistaken for comprehensive knowledge. But this seems to be Socrates' point: although mimetic expertise is exhaustive in its reach, it remains piecemeal in its comprehension. Despite its dazzling capacity for representing anything and everything (an arrogant king, an angry priest, a charming girl, an impertinent fool, a thunderous mob, etc.), the one thing merely mimetic expertise cannot imagine is the governing principle that prescribes the proper place of each of these beings and so provides definition to their community as a whole.

Socrates determines that the poet is properly subordinate to the statesman, despite the fact that the statesman's knowledge is much less exhaustive than that of the poet, because the knowledge the statesman possesses is at least a knowledge of something whole, even if it is by no means finished or complete. Thus, Socrates distinguishes his own enterprise of making a model city from traditional poetry (which is antagonistic to philosophy) by expressly repudiating within his model city the mimetic pretension to exhaustive knowledge. Socrates thus selects as a benediction for his model city Hesiod's line "Half is more somehow than all" (*pantos*, 466c2–3; cf. Hesiod, *Works and Days* 40).[7] Hesiod is deemed here to be really wise (*tôi onti ên sophos*, 466c2)—to possess *real* wisdom, that is, as distinguished from the merely exhaustive (*pantodapon*, 398a1) technical wisdom of the mimetic expert. Real wisdom, which is comprehensive, discerns precisely what is indiscernible in "all." It is in regard to this defect that we may say that half is more somehow than all, for order and wholeness are lacking in a mere totality. For example, a division, disciplined and properly mustered, is more somehow than an entire army routed and scattered in the field. Half is more somehow than all because half, as a ratio, already measures—and thus somehow conveys—the significance of the whole. A random heap (as Socrates regards the feverish city to be) may well exhaust all that a city comprises, but it does so without being *in any way* able to comprehend it. As nothing more than an exhaustive survey of a totality, "all" fails to preserve or to account for the order of the whole.[8]

As a consequence of the founders' imposition of censorship, the model city has undergone an extraordinary change. Thanks to the founders' care in ensuring that its warriors receive an education conducive to civic harmony, the model city has acquired a principle of order. The presence of this *politeia* evidently makes the city sufficiently intriguing to draw Glaucon back into the conversation. Glaucon had stretched himself toward the satisfaction of overwrought, limitless, and fundamentally private appetites in his longing for something more than a merely porcine existence. Yet in so doing Glaucon runs the risk of alienating himself from any possible human community; thus he doubts that the expression "everyone" includes himself (*egô . . . kindyneuô ektos tôn pantôn*, 398c7–8). But when a *politeia* (which is likewise not to be found in the midst of a mere "all") does arise among the interlocutors, Glaucon is able to integrate himself into it. Specifically, he places his own musical know-how at Socrates' disposal in considering which harmonies and melodies are appropriate in the songs to be taught to the city's youth. Glaucon agrees with Socrates that harmony and rhythm must correspond to the words of a song and that these words must fall within the *typoi* already specified (398d4–10). The *politeia*, then, not the art of music, proves to be the founders' guide to what is truly harmonious or musical. Glaucon accordingly joins Socrates in purging all but the Dorian and Phrygian modes, which properly represent the steadfast endurance and measured sobriety characteristic of a really courageous or manly man (*ontos andreiou*, 399a6). The other modes, which from a technical standpoint are no less harmonious than these, are taken away because they are useless to a real man and thus do not harmonize with the city's *politeia*. Glaucon thus discovers that this own manliness, which had caused him to distrust social convention and to isolate himself from community, in fact "fits in" or harmonizes with the model city's *politeia*.

The presence of *politeia* enables Glaucon to contemplate the purified city as a whole and so as something clearly different from the city of luxuriance. In the city of luxuriance, there was nothing to integrate or to inform the diffusiveness of Glaucon's longing for something more than the life of a pig; nor was there anything to delimit the city's own indeterminacy and incompleteness. Glaucon's erotic longing was scattered into a mob of inflamed and limitless desires. Though the purgation of the model city deprives Glaucon of the vicarious satisfaction of his feverish desires (see, e.g., 404b4–e6), it nevertheless informs, reconstructs, and thus holds the prospect of satisfying Glaucon's fundamental eroticism. Plato, the consummate dramatic artist, here delineates a genuine development of character in Glaucon. Only if we keep in mind the distinction between eros and limitless desire, however, will we see that Glaucon's new and surprising sobermindedness (399e7) truly is "in character." Socrates indicates the relevance of this distinction for our consideration of Glaucon by likening the human soul to another "Glaucon," namely, the mythical sea Glaucos, whose original nature is hardly visible because of the mutilations of the waves and the accretions of shells and seaweed (611c7–d7). Inflamed and limitless desires are the mutilations and accretions which presently obscure the original nature of eros. In its unperverted nature, eros is a quickening of the soul responsive to evidence of the beauty of cosmic order and, consequently, is itself measured and musical (403a7–8). It is an erotic quickening of this sort, provoked by the presence of *politeia*, that leads Glaucon to overcome his isolation and to join the others in "being sober-minded." Indeed, Glaucon's membership in the polity of inquiry, including his participation in the purge of the city, is more truly an expression of eros than is his most extreme ultimatum demanding the satisfaction of limitless desire.

There is surely, then, no "abstraction from eros" in the present passage. What occurs is better understood as a first step toward healing or recomposing the mutilation of eros, a mutilation that poses a formidable impediment to the development of Glaucon's capacity for synopsis.[9] The model city's *politeia*, which emerges with the founders' institution of censorship, facilitates the reassembly of the scattered fragments of Glaucon's eros. As a result, Glaucon now recognizes the real man as a member of the model city and thus as belonging to a genuine *politeia*, rather than as a maverick proudly freebooting the thralls of Leviathan. Glaucon realizes that he belongs among the city's founders as a member of the polity of inquiry. Gone, then, are the qualms concerning his inclusion among the others. Instead, we find Glaucon swearing repeatedly in consternation at the discovery of the limits of his capacity to perform his share of the founders' work (400a4, c6)!

Socratic censorship gives form and integrity to the political community primarily by determining what is fitting for the formation of warriors into true civic guardians. Censorship thus counters the danger of civil war, harmonizing the city and adumbrating its integral wholeness. Glaucon's reflection upon Socratic censorship induces a psychic reformation congenial to, but not yet sufficient for, synopsis. Synopsis requires more than a sense of belonging or a recognition of membership. Even among members, there is still the danger that *partisanship* will undermine synopsis. So before entrusting the statesmanly responsibilities of political office to the best of the warriors, Socrates will suggest that they must first pass certain tests designed to challenge their judgment and their power to resist temptations to place partisan interest ahead of the common good. It is likewise not enough that Socrates has overcome the isolation of Glaucon. Although Glaucon now acknowledges his incorporation as a member of the polity of inquiry, he must still be induced to care for the good of this polity in a nonpartisan way. We shall see that the noble lie, which Socrates contrives to address the need for nonpartisan rulers in his model city, will also help to prepare these rulers—and Glaucon himself—for the practice of dialectic.

Nonpartisanship and the Noble Lie

Anything incapable of sustaining itself is not a whole but is merely a part of something else. To be wholly complete, then, the model city must possess the dynamic capacity to sustain itself. So the founders must be discerning recruiters of trustworthy rulers, who must themselves be discerning recruiters of trustworthy rulers. If the city's rulers should allow partisan considerations to bias their judgment in recruiting their successors, they would undermine their city and subvert the common good. Socrates accordingly notes that guardians in the complete sense of the term (*teleous phylakas*) will do their job not out of a concern limited to the interest of some particular part of the city but rather with a view to the good of the city as a whole (428c11–d7). A complete or perfect guardian, in other words, must possess a capacity for nonpartisan synopsis. So the founders must select as rulers those among the guardians who demonstrate this capacity. But who among the guardians can be said to possess a capacity for synopsis? Evidently no one, for Socrates says that he will only *sketch* an answer to this question in the initial foundation of his city; the precise selection of the most perfect guardians will be undertaken later (414a5–7, 503b4–5). Socrates' sketch—his first, rough cut at the task of civic recruitment—merely outlines the trials that are intended to reveal who among the guardians is most reliable in his care for the city.

Socrates would refuse to entrust the city to any candidate who is inclined to be forgetful of his responsibilities, to despair at setbacks, to shrink from terrors, or to be seduced by pleasures. He designs the guardians' trials accordingly, to pose precisely these temptations. Nevertheless, Socrates acknowledges one source of temptation for which he does not construct a test. He recognizes that in addition to other dangers, his cadets are liable to "being robbed" of their convictions by speech (*logos*, 413b6). Yet the examination he designs to test for this possibility involves not words but deeds (*erga*, 413c5–d2).[10] In such a case, one searches in vain for an unobtrusive measure. Of course the definitive trial of one's liability to corruption by speech would involve exposure to an expert rhetorician or dialectician. But the administration of a trial by *logos* would interfere with the proper development of the very trait it is designed to reveal. It is in order to avoid compromising the proper development of his guardians that Socrates eschews dialectical examination. Socrates expressly credits this postponement of dialectic as the feature that makes the model city "altogether opposite" (*pan tounantion*) to any other (497e5–498b3).

Although Socrates' warriors are in other ways tested "as gold is in fire" (413e1–2, 503a5–6), their prudently enforced innocence with respect to dialectic indicates that they are not yet perfect guardians in the most precise sense. Premature practice of dialectic would threaten to undermine the wholesome development of the guardians' nature (538a9–539d7). Nevertheless, these guardians cannot attain completeness—nor can their philosophical nature achieve its fruition—apart from the practice of dialectic (532a1–2). So the founders' postponement of dialectic may be said to highlight the gap between the rulers initially recruited "in outline" and the most precise guardians, the philosopher kings, who are later said to be the proper rulers of the model city (503b4–5). Being in need (*en deonti*, 414b8–9) of something to bridge this gap, Socrates introduces the *noble lie*. It is the lie's function, in other words, to establish the continuity of these developmental stages in the maturation of the perfect civic guardian. Thus, the lie aims first and foremost to propagate complete guardians, not to propagandize the nonphilosophical masses. With this prospect in view, Socrates declares that he will undertake to persuade "first and foremost the rulers themselves" (*malista . . . prôton*, 414c1–d3).[11]

Socrates' lie, though admittedly a deception, is not meant to swindle. Before describing his own lie, Socrates reminds his interlocutors of the lies (*tôn pseudôn*) they had discussed earlier in connection with the guardians' elementary education (414b9). These lies were seen as providing a foundation for human education (376e2–377a7). Socrates' lie is meant to be "one of those lies." As such, Socrates indicates, his lie is a "lie in words," not a "lie in the soul." A lie in the soul, he observes, causes one to be deceived about what really is; as a result, such a lie induces *amathia*, that is, a condition of soul adverse to learning (*peri ta onta . . . amathê einai*, 382b2–3). But a lie in words uses falsehood as a medium of education and so actually enhances the soul's capacity for learning.[12] The lie in words is useful when we are ignorant of the whole truth or when those with whom one speaks are too immature or are otherwise indisposed to assimilate the truth directly (382c6–d3). Such a lie is by no means alien to the truth. On the contrary, Socrates indicates a metaphysical linkage between the lie in words and the truth, remarking that the false and the true represent two aspects of singular *eidos* of words used in the education of youth (376e6–11). Socrates thus envisions a singular and yet twofold eidetic structure, which integrates certain false and true logoi. The singularity of Socrates' *eidos* of words may be said to represent the essential continuity of moments in the unfolding of human education. Such an *eidos* serves as the precondition for the experience of learning. Hence, the appearance of fundamental opposition between truth and falsehood may be recognized as merely an illusion that results whenever one assumes a standpoint outside the province of human learning.

It is with the prospect of just such learning in mind, however, that Socrates describes his lie as noble (*gennaion*, 414b9). Socrates' word *gennaion* may be translated more precisely as "generous," provided we understand this in what lexicographers call its archaic sense, namely, as something both abundantly beneficent and true bred. The beneficence of this noble lie resides in its contribution to the education of perfect civic guardians. This contribution comes by means of a specific misrepresentation concerning the character of the citizens' breeding. And in this regard, the adjective *gennaion* is again well chosen. For it signifies the venerable pedigree as well as the specific theme of autochthonic tales such as the one Socrates contrives in the present passage. Thus, Socrates notes that his lie is "nothing novel, but a Phoenician thing." The allusion may be meant to recall the Phoenician Cadmus, who founded Thebes with the aid of warriors allegedly sprung from the earth; it may refer, at the same time, to the *Phoenix*-like recurrence across time and place of such autochthonic tales. It may refer, in recalling this same Phoenix, to the specific feat of assembling a living whole out of elements as diffuse as ashes or dust. In any case, Socrates' description of the lie as *gennaion* and Phoenician prepares for its detailed formulation, which not only recapitulates the Theban claim of autochthony but in some sense represents the best or truest-bred example of such a falsehood or myth. Specifically, Socrates will undertake to persuade his citizens, and especially his rulers, that what they imagine (*edokoun*, 414d5) to be their rearing and education is only a dream: "I shall attempt to persuade first the rulers themselves and the warriors, and after them the rest of the city, that all these things that they imagined they suffered and had happen to them in our rearing and education were as dreams. While in truth they were at that time being formed and reared within the earth underground . . . and when their fashioning was finally completed, the earth, being their mother, delivered them up" (414d4–6).

But why would anyone attempt to persuade someone that he is dreaming, except to cause him, in some sense of the word, to awaken? Such an awakening is, of course, just what one needs if he is to aspire to a higher plane of intellectual perception. The ramifications of this aspiration are implicit in Socrates' notion of the nature of dreaming; Socrates understands dreaming as a condition in which one believes that a likeness is not a likeness but the thing itself (476c4–8). We may infer that the awakening that Socrates intends to encourage would involve the rulers' ceasing to mistake what they imagine education to be for what education really is. Socrates notes that the citizens imagine their rearing and education to be something that they suffer and that happens to them (*paschein te kai gignesthai peri autous*, 414d5–6); in short, they underestimate the importance of their own contribution in becoming educated. Socrates does not explain why human beings (particularly those who live in well-governed polities) tend to think of their own development as something that happens on the perimeter of their selves, rather than stemming more naturally from their innermost being. We may surmise, however, that the influence of political institutions leaves human beings a little awestruck. (Perhaps this is why Adeimantus takes it for granted that the city is "bigger" than a single man, while Socrates merely raises the question of their relative greatness [368e5–6].) Recognizing this problem, we may say that what is needed, particularly in good polities, is a means of keeping human beings from supposing that their own human nature is nothing but raw material molded by the conventions of an all-powerful culture. The myth of autochthony answers this need in a surprisingly direct manner. In Socrates' rendition, the finished character of mature human beings is presented as the exclusive work of nature and nature's god (415a4–7). Thus, in "the remainder of the myth" (*to loipon tou mythou*), Socrates states that it is a divinity that forms the citizens, a divinity who mixes gold in the genesis of those sufficient to rule, silver in the auxiliaries, and iron and bronze in the farmers and the city's other artisans (415a1–7). On this account, civic nurture plays *no role at all*. Socrates' lie represents the apotheosis of nature, I suggest, in order to combat tendencies toward the apotheosis of the state.

Clearly, it is false to assert that nature in the fullest sense of the word is restricted to genesis or nativity.[13] Any account that presents human nativity as equivalent to human maturity is a lie. Nevertheless, Socrates' lie performs a needed service. It keeps the founders from becoming victims, so to speak, of their own success. For the founder's remarkable achievement in rearing their spirited and philosophical guardians will prove to be counterproductive if it leaves the impression that the statesman's dictate is the essential factor in human development. Rulers especially must be disabused of any such notion. Socrates accordingly offers the noble lie as a means of awakening first and foremost the rulers themselves from the dream (or perhaps better, the dogmatic slumber) of conventionalism. Of course, one may object that this account of the noble lie is somewhat overwrought. For it may be expected that philosophically inclined rulers will on their own be "first and foremost" to see through the illusion of conventionalism. This is true, but it does not go far enough. Socrates goes further. For he predicts that the philosophical youngster will not only see through the illusion of conventionalism but will turn from conventionalism *to its opposite* (*paranomos . . . ek nomimou*, 539a3). Socrates divines that such a youngster will leave off honoring the ancestral traditions he was taught to honor by his political community once he gains so much as a glimpse of what is (538a9–10). Under the stress of an intellectual scrutiny that would reveal the shortcomings of any civic institution, a philosophical soul might well cleave to a more pristine idea of good and evil that can be practiced in any human community. The essential natures of other qualities will likewise be misconstrued as purely abstract principles or patterns, which would only be sullied by contact with whatever comes into being. From this pristine perspective it appears that nothing that comes into being (*ta gignomena*) could ever contribute anything to our understanding of what truly is. Souls inclined to stretch toward such an understanding would develop contempt for *ta gignomena*, including the institutions of their native land. The disillusionment of conventionalism in this way ends in antinomianism if not in outright criminality. In one blow, the city is deprived of properly solicitous rulers and the philosopher (subsequently identified as the perfect guardian) is rendered incapable of the cosmic synopsis prerequisite to his maturation in dialectic.

Antinomianism invites a form of partisanship that we may distinguish from familial or kinship-based partisanship; this form of partisanship is indeed a hazard inherent in the philosophical soul. Socrates indicates that once the philosophical nature catches sight of all time and all being, human life will hardly seem any big thing (486a8–10). The winged souls of philosophic natures "hasten to spend their time always aloft and are unwilling to do what pertains to human beings" (517c8–9). This bias in favor of the class interest of those concerned exclusively with the things "aloft" is indicative of what we may call a *hyperian* partisanship. Socrates' concern for the lack of steadfastness in the philosophical nature thus invites a question: Do the winged souls of philosophers truly soar, or are they merely flighty? The superciliousness of the protophilosopher does not sort well with the generosity characteristic of the truly erotic soul. For the latter "always yearns for the whole and for all things both divine *and human*" (486a5–7, emphasis mine; see also 402a7–d5). If the philosophical nature is not stabilized by a proper nurture and education, its native intellectual keenness will distort its judgment concerning the cosmic as well as the civic polity and ultimately tear it away even from philosophy (491b7–9). The noble lie guards against antinomianism and hyperian partisanship by sidestepping altogether the precariousness of an all-too-easily disillusioned conventionalism. The myth of autochthony motivates loyalty to *politeia* in a manner that avoids the counterproductive ascription of sovereignty to political convention. The sheer *earthiness* of the lie's account of human development helps to keep the feet of Socrates' immature philosophers planted firmly on the ground.

Of course, the political value of the noble lie is self-evident. By playing down the role of civic nurture in favor of the role of authochthonic nativity, the lie ensures that citizens will feel bound to the motherland more closely than could be effected by any notion of convention or social contract. The lie transforms conventional justice into an obligation of piety, fortifying a sense of belonging rather than merely appealing to a merely instrumental and reciprocal obligation. In deference to the divinely sanctioned and natural authority of functional diversity, the rulers "will in no way show mercy if their own offspring is born with an admixture of bronze or iron; instead, they will render the honor appropriate to the child's nature and push it away to the craftsmen and farmers" (415b6–c2). Blood, as we say, is thicker than water; but earth is thicker than either. So the rulers place the interests of their motherland ahead of their own partisan attachment to kith and kin when determining the occupational assignments of their fellow citizens; as a consequence, they will better safeguard the divisions within the city that underlie and fortify its integrity as a whole. It is no coincidence, then, that the task of civic recruitment (which calls for nothing less than this whole-illuminating division of parts) is enjoined as the rulers' "first and foremost" responsibility (*prôton kai malista*, 415b3–4). This formulation echoes Socrates' emphasis on persuading first and foremost the rulers themselves of the noble lie. For the sorting out of human and occupational diversity fortifies the city's constitutional integrity and, at the same time, adumbrates the work of dialectic—and so foreshadows Socrates' judgment that consummate civic guardians need to be philosophers (503b4–5). Even as the lie promotes its immediate political purpose, it is also helping to prepare philosophical and spirited youths for the exercise of dialectic.

Bred on the noble lie, Socrates' philosophical rulers will spurn the cloud-worshiping idolatry of Aristophanes' antinomian intellectuals. Inasmuch as the noble lie helps advance the appreciation of the cosmic significance of *ta gignomena*, it helps to stabilize the intellectual facility characteristic of philosophical natures. Because the founders will not countenance the alienation of the city's most promising youngsters from *ta gignomena*, the intellectual advance of these youngsters will more likely culminate in a comprehensive grasp of the cosmos as a whole. On the basis of this cosmic synopsis, they will be able to undertake the dialectical examination of their own upbringing and ancestral beliefs—including the noble lie itself—without becoming careless or contemptuous of their city's institutions or their fellow citizens. Thus Socrates' choice of words is precise when he says that it is as "nurselings" (*trophimoi*, 520d6), that is, as beneficiaries of the city's nurture, that his philosophically inclined youngsters will see that they are not a class apart but are themselves members of a cosmic polity, a polity that transcends yet comprehends the civic polity.

Glaucon *andreiotatos*, most manly and courageous always and everywhere, finds the noble lie appealing because its underlying message is that one need not kowtow to *nomos* to be loyal to *politeia*. The lie counters the tendency of well-reared human beings to become captivated by good *nomoi*. The thralls of *nomos* will never acquire a truly mature human excellence even under the most salutary conditions; if given the chance, they would choose to be tyrants (619b7–d1). Glaucon and the best of the youths of the model city will see the falseness of the lie. Yet they will also see its nobility—its generosity in providing or preserving a way to the truth. Once Glaucon sees that a lie—a thing far different from the pristine truth—can serve an essential educational purpose, he can begin to overcome his feelings of contempt for appearances. He, and the city's philosophical youth, can begin to recognize the counterproductive immaturity of their own hyperian zeal for proceeding directly or immediately to the truth *auto kath' hauto*. Glaucon's support of the noble lie represents a growing appreciation of the importance of images and appearances—and even falsehoods—for the seeker of truth. Glaucon and other philosophical youth are likely, as a result, to take a less haughty view of the human beings who occupy the realm of appearances. This more moderate approach is prerequisite to Glaucon's own transcendence of hyperian partisanship. It is necessary, moreover, if he is ever to acquire a mature righteousness or human excellence.

As we have seen, Socrates devises his noble lie because his city requires rulers whose judgment is nonpartisan. The lie helps philosophical rulers to cultivate a capacity for synopsis no more distorted by concern for the "things aloft" than for kith and kin. But, before Socrates is willing to regard the foundation of his model city as complete, he addresses one more obstacle—perhaps the most difficult and surely the most intimate obstacle—to the beneficial practice of dialectic. This obstacle derives from the peculiar sense of isolation that we feel in our own bodies.

Socratic Communism and the Battle against Erotic Idiosyncracy

With the completion of the guardians' education in music and gymnastics and with the additional safeguard of the noble lie, one might think that the foundation of the model city would be complete. But Socrates warns Glaucon that one must still be on guard lest the auxiliaries, who are meant to be the city's watchdogs, turn wolf and attack the flock (416a2–b4). The education that has been given to these men aims to provide them with the most important safeguard (416b1–c4). But Socrates observes that "someone intelligent would say that, in addition to this education, one must provide them with dwellings and such other substance as will not stop them from being the best possible guardians" (416c5–d1). Socrates accordingly proposes that the guardians shall possess "no private substance whatever, unless it is entirely necessary" (416d5–6). What is necessary for guardians is different, of course, from what is necessary for farmers. Private ownership of land is for farmers an instrument of membership in the polity. The possession of such property encourages efficient agricultural cultivation and thus ensures that farmers will not cease being farmers (417a6–7). But Socrates' warriors are to be civic guardians, not farmers. While the private ownership of boots, toothbrushes, and other such necessities might fit perfectly well into the guardian's role in the civic *politeia*, the possession of private estates, houses, and money would only be a hindrance to the warriors' distinctive work of caring for the city as a whole and so must be proscribed. In the model city, property (that which is one's own) consists in nothing else than the equipment appropriate for performing one's part in the *politeia*.

Although Glaucon endorses Socrates' proposal, his brother Adeimantus ventures a criticism. We have not heard from Adeimantus since he joined Socrates in banning insubordinate poets from the city. Now it is apparent that Adeimantus has some unresolved difficulties with synopsis. Adeimantus wonders how Socrates would defend himself if someone were to charge that he had denied happiness to his guardians, the very men "whose city it is in truth," inasmuch as they are not permitted to possess great and beautiful houses or to have the opportunity of private sacrifices to the gods or because they cannot embark upon trips abroad in private or make presents to girlfriends (419a1–420a8).[14] The problem, according to Adeimantus, is that the guardians are more like foreign mercenaries than the rulers of any recognizable city. This is a telling criticism, at least as far as Adeimantus himself is concerned. For in the course of its formulation, Adeimantus reveals his own presumption that the city belongs to its rulers. He has forgotten that the truth about *politeia* is quite the reverse: it is not the city that belongs as private property to the guardians but the guardians who, along with the other citizens, belong as members to the city. Adeimantus needs to be reminded of his responsibilities as a member of the team of interlocutors founding the model city. For the work of answering such indictments, Socrates indicates, is not Socrates' private burden (as Adeimantus had suggested) but is rather a task shared in common with his cofounders (cf. 419a2 and 420b1). Adeimantus' fretting over the lack of private gratifications suggests that he, at least, has not yet benefited sufficiently from Socrates' *propaideia*.

Adeimantus' problem is that he is not attending to the model city as a whole. His concern for the role of private indulgence in the constitution of individual happiness stands in the way of his successful practice of synopsis. So Socrates, in response, reminds him that the founders' task is not to seek the exceptional happiness of any tribe within the city but rather to consider how it is that the city itself may be, as much as possible, a whole (*holê*, 420b5–8; *holên*, 421b6). It is in such a city (i.e., a whole city, *holês poleôs*) that the interlocutors had thought they might find justice (368e3, 420b8–9). The political institutions of the model city serve the discovery of justice by constituting the civic community as a whole. This purpose, as Socrates reminds Adeimantus, must be kept clearly in mind if one is properly to appreciate the founders' communistic proposals.

Adeimantus accepts Socrates' reminder (421c7). He agrees with Socrates that the most beautiful sign of civic improvement—the noblest limit of civic enlargement—is the achievement and maintenance of political wholeness or integrity. The interlocutors agree, moreover, that if the guardians' education and rearing ("the one great, or rather sufficient, thing") is safeguarded, the guardians themselves will be equipped to discern all that is further required for the city to reach the fullness of its growth. These further requirements are left undetailed for the time being. Yet they too must be comprehended by any synoptic view of the civic *politeia*. Thus Socrates makes a passing reference to these requirements, noting simply that the possession of women, matings, and procreation should all be arranged to conform as closely as possible to the proverb "friends' affairs are common" (*koina ta philôn*, 423e5–424a2). Socrates postpones the elaboration of this proposal for good reason. Not only will the details of such a proposal stir up a hornet's nest of opposition, but its elaboration as a whole will involve the very dialectical examination (*dielesthai*, 449d3–450a2; *dialegesthai*, 454a4–d1) that he has been so carefully postponing. The three waves of book 5, which Socrates and his interlocutors will confront and eventually survive, are illustrative of the dangers that anyone engaged in a trial by dialectic necessarily faces. But Socrates must still doubt whether his interlocutors are sufficiently prepared to take the risks involved in such a dialectical examination (450d8–451b8). Adeimantus' anxiety concerning private indulgence suggests, as much as anything, that more preparation is needed. It is as a final stage in his dialectical *propaideia*, we may infer, that Socrates offers this muted foreshadowing of the communization of family life.

Like the other controversial political institutions that we have already considered, Socrates' connubial communism—just so far as it is developed through the completion of the foundation (*ôikismenê*, 427c6–d1) of the model city—contributes to the fortification of the interlocutors' capacity for synopsis. (In recognition of the distinction between this propaedeutic and the actual practice of dialectic, I reserve discussion of the barnyard analogies and the other features of connubial communism as detailed in book 5 for another occasion, when the whole matter of the reintroduction of dialectical examination may be appropriately considered.) So we must concentrate our investigation here upon the only clue concerning the character of Socrates' connubial communism that is provided prior to the completion of the city's foundation, namely that the possession of women, matings, and procreation should be arranged to conform as closely as possible to the proverb "friends' affairs are common." We should begin by noting that Socrates introduces his endorsement of this proverb—and its surprising application to sexual mating and procreation—as a counter to the tendency toward self-indulgence and self-centeredness, which is implicit in the indictment brought by Adeimantus.

Beside the more spirited and hyperian sources of alienation or partisanship already mentioned—which seem to be at the root of Glaucon's characteristic difficulties—each of us is continuously confronted by that most tangible reminder of human separateness: his or her own body. As is clear from the differences between the truthful city and the feverish city, Adeimantus is temperamentally much more solicitous of the human body and its natural requirements than is his brother. Indeed, for the mass of human beings, the indisputable separateness of the human body and the exclusivity of individual sensory impressions suffice to demonstrate that the body is radically private. Narrowly focussing upon the indulgence of the bodily senses, human beings become even more credulous of assertions that our bodies and what we do with them are nothing but "our own affair." (Perhaps it is for this reason that Plato makes the conversation reported in the *Republic* occur as an alternative activity to the feast and all-night orgy taking place concurrently in the Piraeus. The interlocutors' "being moderate" both in speech and in deed necessarily precedes their consideration of the significance of the body.) Now the claim of individualistic self-indulgence is especially compelling in the most physically intimate relationship of all, the sexual mating of man and woman. Yet if mankind is even more a coupling animal than a political animal, it would seem that the opportunity for synopsis in connection with the connubial *politeia* presents itself ahead of all others. If one fails to achieve synopsis in this matter, it is quite unlikely that he or she will do much better in the case of civic or cosmic synopsis. So Socrates has good reason to speak in earnest when he proposes that sexual matters be arranged to accord with the proverb "friends' affairs are common". For nowhere are the problematic status of the separateness of our bodies or the obstacle this poses to the achievement of synopsis more evident than in connection with sexual matters.

There is, of course, nothing quite like sex—and not merely in terms of intensity of pleasure. For other bodily appetites yield to perfectly appropriate private satisfactions. Consider, for example, the appetite of hunger. There is no breach of civic membership, nor is there anything partisan, when a guardian ingests and thus privatizes the moderate ration of food that he eats. The exclusive ingestion and assimilation of this food is precisely what should take place if the guardian is to be a guardian. In fact, Socrates makes a point of endorsing the Homeric practice of award-ing the best cut of meat as a prize to the best warrior. Such an award is especially fitting, Socrates says, because the best warriors thus derive both honor and an increase in strength at the same time (468c10–d6). This remarkable combination of results is possible because the purpose served by hunger is to facilitate the strength or health of one's own body. Food, which exists to satisfy hunger, can be used for the purpose for which it exists only by being made private. In this regard, sex is rather more complicated. Of course, the pleasure obtained in the satisfaction of sexual appetite is as exclusively private as any other sensory impression. Nevertheless, the chief purpose of sexual intercourse is procreation; and the work of procreation is by no means a private or exclusively individual affair. In procreation, the intercourse of two (the Greeks called this intercourse "being-together" or *synousia*) is needed to generate a one. The coming into being of any single human body, whether male or female, presupposes the *synousia* of male *and* female. We may go so far as to say that it is impossible to understand the body apart from its role in a connubial partnership. For the elemental fact of sexual differentiation and complementarity is unintelligible in any other way. We shall never appreciate the body rightly if we consider it reductively, as nothing but an idiosyncratic bundle of nerves. If, instead of such reductionism, one grasps the body as a whole—and this is perhaps the primordial instance of synopsis—one cannot fail to observe that the body is naturally either male or female and thus the natural complement of a body of the opposite sex. The *synousia* of male and female thus constitutes a whole toward which each is erotically drawn. Yet sexual individualism treats the body as if it were nothing but an instrument of sensation. Erotic idiosyncracy thus undermines the intelligibility of the body and obscures the synopsis of the connubial partnership as a whole. The sexual differentiation of the human body—its inflection into virile male and fertile female—suggests that at its most fundamental the human body is not simply "one's own affair"; the body is a sign and at the same time the instrument of a determinate and whole-seeking erôs, rather than of a limitless and merely self-indulgent appetite. Hence, we may say that the assertion of a purely private prerogative in sexual matters is indicative of the failure synoptically to grasp the connubial significance of the sexual character of the human body. Though we do not yet know the precise details of Socrates' reformed *nomos* for sexual mating, we may expect that it will be intended to combat self-centeredness by encouraging more attention to the connubial significance, as distinguished from the physiological separateness, of the human body.

The assertion of individual prerogative in sexual matters obstructs the achievement of a synoptic view of the human body and so obscures the *politeia* that properly obtains in all the relations (not just the sexual ones) between man and woman. In short, erotic idiosyncracy undermines any prospect for friendship between men and women. Connubial communism may be necessary, from Socrates' perspective, because there are not sufficient safeguards against erotic idiosyncracy in the sexual matings undertaken currently in the context of private family life. Exclusivity in the mutual espousal of man and woman may be understood as pointing toward connubial synopsis, but only if one is already so inclined. For someone differently inclined (e.g., toward somatic self-centeredness and erotic idiosyncracy), marital exclusivity alone will not counter the prejudice that sex exists for the sake of individual satisfaction. Of course, the most congenial institution for the erotic individualist is the "singles bar." In a world in which such an institution had itself become normative, the paradoxical proposal of sexual exclusivity might well encourage a reconsideration of connubial synopsis.[15] But as far as Socrates and his interlocutors are concerned, sexual exclusivity remains the norm, and the privacy of family life conforms itself all too easily to erotic idiosyncracy. Inasmuch as erotic idiosyncracy undermines the achievement of connubial synopsis, there is little doubt but that it will also corrupt and disable the soul's capacity for grasping as a whole any superordinate community of which man and woman are a part. Socrates' assault on this form of individualism thus aims to orient his interlocutors toward the wholeness of the connubial partnership and any other partnerships, such as the political community, to which the connubial partnership belongs. As long as the connubial significance of the human body is neglected, the dialectical examination of questions such as whether the just or the unjust are happier will only endanger the excellence of the human soul. In the interest of preparing for such an exercise of dialectic, however, Socrates would have us replace individualism in our understanding of sexual matters with an emphasis on sexual community. His strategy is evidently successful, at least to this extent: when Adeimantus formulates his second thoughts on the question of connubial communism and the rest of the women's drama, he speaks synoptically of Socrates' stealing a "whole *eidos* of argument" and expresses his view that "the whole community of women and children makes the whole difference to a *politeia* coming into being rightly or not" (449c2, 449d3–6).

CONCLUSION

Glaucon is in a precarious position, as we meet him in the opening of Plato's *Republic*. His native inclination is skewed toward justice but he is, nevertheless, fascinated by injustice. Perturbed by the inconclusiveness of Socrates' response to Thrasymachus' defense of the unjust life, Glaucon resolves to place his own native decency in jeopardy in order to acquire a mature human excellence. Spurning the conventionalsim of Thrasymachus, Glaucon argues that a real man—the true individual, who feels himself in no way beholden to the political community—will find injustice naturally superior to justice. This, at least as Glaucon sees it, is the argument that must be answered if any of us is to choose justice for its own sake, rather than merely being forced by circumstances to accept justice as the price paid to avoid suffering injustice. He accordingly challenges Socrates to show the superiority of justice in a single soul, apart from all that a reputation for justice might convey.

But as Adeimantus suggests, our souls have been distorted by the influence of traditional patterns of childrearing. As a consequence of this distortion we are not in a condition to make the sovereign choice that Glaucon assumes as his prerogative. What is lacking in our souls, moreover, cannot be supplied by the mere addition of further argument. Adeimantus indicates that the soul "turned wholly to appearances" suffers from a distorted view of the question of justice and injustice; a soul thus twisted will always find that injustice is superior to justice, no matter the argument. So this psychic distortion must be corrected before the bearing of any further argument upon the choice between justice and injustice can be determined. Nor will the deformity of a soul turned wholly to appearances be remedied by turning exclusively toward "what is." Such a turn (like the one attempted by Glaucon at the opening of book 2) does not correct but merely inverts the warp of the distorted soul. What is needed, instead, is for the soul to develop an ability to comprehend as a whole both what is and what appears. Socrates expressly identifies such a whole as "the most beautiful" kind of good (358a1–3). The comprehension of *this* good would entail the synoptic capacity that Socrates declares to be prerequisite to the beneficial practice of dialectical argument.

In order to facilitate the development of this necessary synopsis and to forestall any further fruitless or even counterproductive dialectic concerning justice, Socrates proposes that his interlocutors join him in making a model city. Socrates' political proposals are not meant, then, as a blueprint for public policy; nor is it their principal intention to critique or to parody political idealism. Each of the city's controversial institutions contributes to the remediation of psychic distortion and fosters the soul's capacity for synopsis.

Censorship fosters an appreciation of *membership*. The feverish desires of the inflamed city represent the diffusiveness of the individual soul's longing in a condition anterior to the achievement of constitutional synopsis. The civic polity itself emerges only with the introduction of censorship. Glaucon, who regarded his individual self as "apart from everyone," discovers to his own surprise that he is growing more sober-minded as he affirms the principles of Socratic censorship. Thanks to this censorship, Glaucon comes to see himself as a part of the whole rather than apart from all. It bears repeating that Socrates' proposal for censorship is not a call to arms against the freedom of speech and expression found in presently existing political communities. To respond to such a call would only distract us from the point of Socrates' exercise. Socrates' discussion of censorship is not meant as a public policy proposal but as therapy for better comprehending constitutional polity as a whole.

But synopsis requires more than an appreciation of membership, for membership still allows for the possibility of *partisanship*. Socrates contrives his noble lie to counter partisan tendencies, "especially [among] the rulers themselves," that inordinately bias judgment either in favor of kith and kin or "the things aloft." The noble lie—a medicinal lie in words, as opposed to the hateful lie in the soul—helps the rulers to perform their task of civic recruitment in a wholesome (i.e., a whole-revealing and whole-preserving) manner. It also provides a medium essential for continuity in the procession of philosophical education. Apart from the integration of this learning experience, even well habituated youngsters will end in repudiating the very persons and customs that sustained their first experiences of education and will turn tyrant at the first opportunity. The lie is thus the linchpin that holds together the city of warriors with the beautiful city of philosopher kings. It underlies and holds the key to the integrity of Socrates' model city through the major stages of its development.

Socratic communism addresses certain tendencies toward self-indulgent individualism, which are rooted in the separateness of one's own body. The inappropriateness of private possession of lands, houses, and money by civic guardians is indicated first of all. Although such things are needed by farmers and householders in the course of performing their civic role, these possessions would only distract civic guardians from their proper jobs. But communism culminates, for Socrates, in an assault upon *erotic idiosyncracy*. The self-centeredness of erotic idiosyncracy is countered by Socrates' application of the proverb "friends' affairs are common." Above all, the intelligibility of the human body itself depends upon an appreciation of its connubial significance in procreation. To attain this perspective we must, of course, transcend the reductive perspective that views the body as nothing but an individual bundle of nerves. Bodily sensation, we may grant, is exclusive and hardly communicable. But the body itself, which bears unimpeachable evidence of the complimentarity of male and female, is only mistakenly regarded as a private affair. The objective of Socratic communism is to counter this mistaken tendency and to illuminate the connubial (and thus the civic and even the cosmic) significance of the body. The tendency to treat the human body as idiosyncratic renders the body unintelligible and poses an impediment to connubial, civic, and cosmic synopsis. Once again the point of Socrates' correction of the distorted view is neither to promote nor to discourage social change but rather to foster the synopsis prerequisite to the deliberate and reasoned adoption of a polity in one's soul.

The investigative strategy of Plato's *Republic* suggests that it is prudent, before choosing a polity for one's soul, to get some experience by establishing a polity for something less precious—for example, a city in speech. The foundation of the model city thus serves the purpose of fostering the interlocutors' synoptic capacity as a prerequisite of their mature choice of life. Is it fitting, then, to maintain that Socrates' polity is the best city? If the function of the political community is to assist persons in achieving virtue, then it seems that it could count as such—though it never see the light of day. The existence or possibility of this city is irrelevant and can be seen to be so once one has understood what its true purpose is.

Notes

1. Citations in this essay to the *Platonis opera* (Plato 1900–1907) follow the customary Stephanus notation. Unless indicated otherwise, all citations are to the *Republic*. The translations are my own.

2. Thus I am in agreement with Strauss when he notes that in interpreting Plato "one cannot take seriously enough the law of logographic necessity. Nothing is accidental in a Platonic dialogue; everything is necessary at the place where it occurs" (1964, 60). It should be understood that "everything" includes the dramatic action as well as the arguments of the dialogue. Those who ignore dramatic evidence in their haste to dissect "Plato's" arguments labor in vain. But I also agree with Klein that "we [readers] are one of the elements of the dialogue and perhaps the most important one" (1965, 9). In this sense, the completion (i.e., the perfection) of the work intended by Plato is always up to us and so is never quite beyond the reach of chance. Consequently I would say that freedom and necessity are in perfect balance whenever learning takes place through the medium of a Platonic dialogue and the author's intention is thus fulfilled.

3. In Plato's *Laches*, Socrates explores the bearing of courage on the character and the pursuit of human excellence. For further discussion of this dialogue, see Dobbs 1986.

4. The definition of what exactly constitutes *one* job is, for the time being, deduced from the singularity of a given art. Later, the founders find that the two arts of music and gymnastics constitute *one pair* appropriately assigned to those who are to undertake the job of civic guard (411e4). It follows that *technê* no longer provides a sufficient indication of singularity of occupation. The founders' recognition of the integrity of this pair of arts thus represents a preliminary achievement of synopsis. Nevertheless, a certain unwitting dogmatism, which implicitly assumes that certain jobs or practices are *unintegrable*, has led even careful students of the *Republic* to maintain that Socrates' proposal for a philosopher king "rejects in its very formulation the principle of one man, one job" upon which the model city is based (see, e.g., Benardete 1989, 180). If nothing else, Socrates' appreciation of the *questionableness* of what constitutes *one* or makes for integrity (524e2–525a5) should give us pause before dismissing the integrity of the philosopher king on such simplistic grounds, as Strauss, for example, rightly perceives (1964, 101). For an interpretation that attempts to make the case for the integrity of Socrates' philosopher kings, see Dobbs 1985b.

5. Socrates thus notes that it is not only mimetic experts or insubordinate poets who are to be censored. Eventually, every craftsman is supervised to determine who is best able to follow the track of beauty and goodness of form; only those with this ability are permitted to remain in the city (401c4–d3). Others will be ejected, notwithstanding their specialization. Although the pastry chef, the relish maker, and the courtesan were quite welcome and appropriate in the city of luxuriance, all these specialists must now be purged. For their products and services contribute nothing to the education of the warrior youth, which is necessary if the city is to possess a *politeia*. We may say that the rule one man, one job or art still prevails. But the meaning of oneness or integrity is enlarged beyond the mere consideration of specialization. Integrity of craft (and even integrity of function) is determined by harmonization with the *politeia* of the model city.

6. The truly epic pretentiousness of the mimetic expert is clear enough in the text before us. But for further testimonials, consider the whole of Plato's *Ion* and, in particular, the following speech of Niceratus from Xenophon's *Symposium*: "You are aware, of course, that the supremely wise Homer has made poems on nearly every human subject. So anyone of you who wishes to become a skilled householder, politician, or general—or to be like Achilles, Ajax, Nestor, or Odysseus—should attend to me because I know all these things!" (4.6). (My thanks to J. Budziszewski for calling my attention to this passage and for many helpful remarks on an earlier draft of this essay.)

7. The true significance of Socrates' citation of this line has eluded readers, I suspect, because Hesiod is taken to be asserting that "the half is more somehow than the *whole*." Such a rendering is linguistically tenable, yet it yields no sense. For there is, I maintain, *no way* that half is more than the whole. I suggest that in interpreting Hesiod's dictum, we instead take our bearings from the distinction between piecemeal exhaustiveness and comprehensive understanding, which I argue is crucial to Socrates' discussion of censorship. Then we may see that Hesiod's word *pantos* should be rendered as "all" rather than as "whole." Half is indeed "more somehow than *all*" in that a mere totality lacks the order already adumbrated in the recognition of half. By approaching the interpretation of this line in this way, I maintain, the reader is able both to make sense of Hesiod and to appreciate the appropriateness of Socrates' citation.

8. Thus I hold that the synopsis implicit in recognizing that "half is more somehow than all" does not require that one first come to know *all* the beings. The kind of reasoning that contemporary psychologists and philosophers of science identify as "pattern recognition" entails the possibility of cosmic synopsis apart from an exhaustive survey of all cases (see, e.g., Hanson 1965, 85–92). Hence, I cannot agree with the Speusippean interpretation of "cosmic synopsis" cited by Brann (1967, 116) and followed by many other scholars.

9. Compare Strauss's observation that "The *Republic* could unqualifiedly abstract from *erôs* only if it could abstract from philosophy" (1964, 112).

10. Adam overlooks the significance of Socrates' exclusive reliance upon works or deeds (*erga*, 413c8) in conducting his tests. Hence, he considers it "a curious fact that Plato's *klopê* [the loss of true opinion by theft, including theft by speech] still leaves a loophole by which vicious poetry may creep in again. On the general question, Plato does well to insist on the educational value of temptation; the theory and practice of modern times recognizes it in connection with *bia* [force], but experience too often shows that *klopê* and *goêteia* [wizardry] mean playing with fire" (1902, 1:191–92). Of course Adam is quite right "on the general question." Experience does show that such things mean playing with fire, as Socrates clearly agrees (537e1–539d7). It is for precisely this reason, I contend, that Socrates restricts the testing of his warriors to works (*erga*), not words.

11. So it is by no means Socrates' leading concern to dupe farmers and artisans (and nonphilosophers generally) into servile obedience. Yet one finds that the vast majority of commentators on this passage (citing them by name would serve no useful purpose) vainly invoke anachronistic political sensibilities to denounce Socrates' lie as a propagandistic outrage. Strauss, Bloom, and others influenced by their interpretation, must be considered separately, however. For they at least do not assume the superiority of the modern sensibility (Bloom 1991, 93–94; Strauss 1964, 102). On the contrary, they go so far as to endorse the lie. But they do so exclusively on grounds that the need for propaganda is inescapable. So their account, too, ignores the pedagogical relevance of the lie for the rulers themselves. As Strauss sees it, political order will always depend upon deception, inas-

much as the multitude is necessarily unphilosophical and therefore unwilling to consent to be governed by the truth. Thus he writes, "It is therefore with special regard to the ruled . . . that Socrates introduces at this point the noble lie" (1964, 102). It is on the authority of this interpretation, we may surmise, that Bloom, in his translation of the *Republic,* tones down Socrates' express statement that it is *first and foremost the rulers themselves* who are to be persuaded. (Bloom's rendering of *malista* as "in the best case" [414c1] sounds hopeful rather than emphatic. Moreover, he elides the intensifying pronoun *autous,* which Socrates repeatedly employs [414c1, d2] to stress that it is the rulers *themselves* who are the principal target of the deception. Bloom's rendering of this key passage thus tends to overstate the tension between truth and the political community and between philosophy and the citizenship of the best ruler). Voegelin, by contrast, deserves credit for taking seriously Socrates' indication that it is the rulers themselves who are to be persuaded by the lie (1957, 104–8). This is not to say, however, that what Voegelin takes to be lesson of the lie is exactly what Socrates had in mind.

12. For Socrates it is this contrast between being deceived and being swindled that is fundamental. Some lies are medicinal in contributing to the soul's healing and recovery of a capacity for learning, others are poisonous in inducing a condition of soul antithetical to the experience of learning. This opposition, not the over-parsed distinction of lie and myth, is most critical from Socrates' point of view. In fact, Socrates twice refers to the noble lie expressly as a *mythos* (415a2, c8).

13. For a discussion of the importance of this distinction for classic natural right, see Dobbs 1994.

14. Adeimantus' criticism is indicative of a synoptic deficiency, so it should not be confused with Aristotle's contention that Socrates deprives the guardians of happiness in his own interest (*Politics* 1264b15–17). The crucial difference—a difference that elevates Aristotle's treatment of the *Republic* far above the critical assessment it has typically received from its commentators—is that Aristotle's critique issues from a truly synoptic appreciation of Socrates' undertaking in the *Republic.* For further discussion, see Dobbs 1985a, 29–34.

15. Compare Harry Neumann's observation: "Only in Socratic communities would happy marriage be more erotic than adultery. . . . Thus Socrates in Plato's *Republic* advocates a policy which publicly forbids normal marriage and family life" (1973, 452).

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American Political Science Review Vol. 88, No. 2 June 1994

HEIDEGGER ON FREEDOM: POLITICAL NOT METAPHYSICAL

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This essay analyzes modern and postmodern concepts of freedom and contrasts them to a Heideggerian understanding. Positive, negative, and what might be called Foucaultian or Nietzschean liberty are demonstrated to bear a common trait. In such modern and postmodern formulations, freedom is consistently identified with a form of mastery. This identification of freedom with mastery, I argue, encourages ecological abuse, supports the dangerous prerogatives of statist sovereignty, and strengthens the resilience of patriarchy. The political significance of Heidegger's alternative vision is addressed.

In 1958, Isaiah Berlin delivered his now famous essay "Two Concepts of Liberty" (Berlin 1969, Chap. 3). From the hundreds of senses of the words *liberty* and *freedom* that historians of ideas have recorded, Berlin selects two for analysis: "positive" and "negative" liberty. Such timely conceptual distinctions, Berlin insists, have significant political ramifications. Attending to ideas in order critically to assess their meaning may prevent their acquiring an "unchecked momentum and an irresistible power over multitudes of men that may grow too violent to be affected by rational criticism" (p. 119). Berlin is reflecting on Heine's observation of the relationship between Rousseau's thought and Robespierre's deeds and on Heine's prediction that the romanticism of Fichte and Schelling might one day turn the Germans against the liberal culture of the West. More to the point, Berlin had the then closing Iron Curtain on his mind, and he would critically evaluate positive liberty as its ideological foundation.

With a similar concern for the social and political effects of our thinking, I propose to reexamine the concept of liberty. My worry, however, is not with past fascist threats or looming totalitarian menaces to liberalism. I am more concerned with liberalism's very success. To be specific, I am worried about the danger posed to our social and political life, our world and our earth, by the prevalent notions of freedom that underlie Western political culture. My intent here is not to add to the prodigious debate surrounding Berlin's seminal essay but to employ his rather facile schema, without the criticism it deserves, for other purposes. The analysis will not offset positive with negative liberty, but positive liberty, negative liberty, and their postmodern counterpart, with an understanding of freedom for which Martin Heidegger serves as the spokesperson.

Heidegger is seldom acknowledged for his concern with freedom. There are chiefly two reasons. First, Heidegger articulates freedom in a way that takes us beyond traditional formulations—formulations to which he remains consistently critical. As such, either his discussions are considered overly idiosyncratic and hence irrelevant to standard debates or his perspective (particularly given his understanding of technology) is held to leave little room for liberty of any ilk. Assessing Heidegger's relation to freedom as the Western world's preeminent political value, one recent commentator would thus dismiss him out of hand, observing curtly, "You can't be a very effective spokesman for freedom when your philosophy tells you that it doesn't exist" (Crews 1992, 33). I believe, however, that freedom constitutes the core of Heidegger's thought.[1] Indeed, Heidegger himself insists that "the question of the essence of human freedom is the fundamental question of philosophy, with even the question of Being entwined in it" (Heidegger 1976–89, 31:300). What follows is an effort to demonstrate the meaning and political relevance of this claim.

The second reason accounting for the frequent dismissal of Heidegger's contribution to our understanding of freedom is his own political biography. For many, Heidegger's work is irredeemably tarnished by his fervent support of national socialism while serving as rector of Freiburg University beginning in 1933 and by his stubborn reluctance after the war to come to terms with the meaning of this involvement. Heidegger's political biography and its relation to his philosophy has proven material for volumes of discussion and diatribe. Here I have chosen to skirt this issue—maintaining a theorist's prerogative to derive from a philosophy political sensibilities that its author has not earned and might well disown. The political implications I draw from Heidegger's understanding of freedom, to be sure, are in conflict with the more infamous aspects of his life. Yet they are congruent, I would argue, with the thrust of his philosophy as a whole. Moreover, my effort remains faithful to the basic orientation of Heidegger's own investigations. In philosophy, Heidegger observes, discovering necessarily means shaping (1985a, 38). Like all works of interpretive political philosophy, mine shapes a certain world out of something discovered in another's text. My concern, then, is not "with 'Heidegger', but with what he thinks about" (Blitz 1981, 16). I argue that in thinking about human freedom, Heidegger, after some missteps, demonstrates the need for a fundamental rearticulation of what in the West we find most essential to our historical and political being. How to voice this rearticulation of freedom within one's own political life is, of course, neither for Heidegger nor for his biographers to decide.

FREEDOM—NEGATIVE, POSITIVE, AND POSTMODERN

Negative liberty, simply put, is freedom *from* constraint. It signifies the (political) space accorded the individual to pursue desires unhindered by the impositions of others. Its particulary modern formulation, stemming from the Renaissance and Reformation, is the notion of privacy—a sacrosanct area of personal freedom over which the individual has complete jurisdiction and which demands protection from all external interference, whether that of the church, state, or society. Negative liberty denotes the individual's unchallenged control over his immediate environment, enabling the exclusion of others from its trespassing. Berlin identifies negative liberty as the core of liberalism. Positive liberty, in contrast, is not a freedom *from* but a freedom *to.* It denotes a freedom to do. But positive liberty entails doing not only what one desires, unhindered by external constraints, but what one *should* desire, unhindered by internal constraints such as irrational drives, weaknesses of character, false consciousness, or even shortsighted judgment. Free will is only truly free, in other words, when it actualizes the individual's "objective" interests. Hence, positive liberty necessitates both the discovery of one's proper needs or desires and the capacity to see them fulfilled. If, with Rousseau, we discover our true interests to coincide with the general will, then we may find ourselves in the curious position of being forced to be positively free. If, with Marx, we find our true interests to be determined by our nature as species beings, then attaining positive liberty consists in achieving radical equality and fraternity such that each gives according to his abilities and receives according to his needs. And finally, if, with the Nazi ideologues, we hold our true interests to lie with the expansion and rise to power of the *Volk,* then we may experience our positive liberty only in the life and death struggle for *Lebensraum.* Positive liberty, in short, is freedom to be the most one can be. Attaining this goal—often a group effort—entails both the discovery of one's higher self and the extension of its mastery over one's lower (deluded) self.[2] Berlin therefore identifies positive liberty with self-mastery. As an isolated ideal, self-mastery would appear difficult to disparage. The problem is that the selves being mastered within the social whole are generally understood to be identical in desires, needs, and reason. Historically, Berlin notes, the quest for positive liberty has therefore led to a "prescribed form of life" that often serves as "a specious disguise for brutal tyranny" (1969, 131).

Berlin, it comes as no surprise, opts for negative liberty. He advocates for the pluralism it assumes and fosters. Positive liberty, he acknowledges, has its place, but it is a place of subordination to its negative counterpart. The positive liberty to participate in government is consequently identified not as an end-in-itself (citizens' realizing their full political nature) but rather as the only tried and true means of protecting negative liberties. Here Berlin appears to echo Samuel Johnson's reported view that "political liberty is good only so far as it produces private liberty" (Boswell 1979, 140). Yet Berlin is not a radical libertarian. He means to argue against the conflation of meanings and values, and he deprecates the tendency to place negative liberty upon an unassailable pedestal. Freedom is freedom, and it is good. But it is not the only value we have or should have. Justice is also good, and its instantiation might at time require a diminution of (negative) liberty. To underline the point, Berlin observes that we force children to go to school. In so doing, we constrain their freedom for the purpose of elevating another value, namely the diminution of ignorance. To say that in forcing children to attend school we are not actually curtailing their freedom because their freedom is effectively increased by the growth of their knowledge (of possible choices to be made, etc.) is only dangerously to conflate the meanings of positive and negative liberty.[3] One senses that in this regard Berlin would agree with Edmund Burke. "Liberty," Burke writes in his "Letter to the Sheriffs of Bristol," must be limited in order to be possessed" (Burke 1960, 211).

The concepts of negative and positive liberty, Berlin proposes, gave rise to "the great clash of ideologies" that dominated his time. In 1958, Berlin could write that the view of those who reject negative liberty and favor its positive counterpart "rules over half our world" (1969, 138, 141). Concerns are significantly different today. History has rerouted the Marxist yearnings for positive liberty and swept away their institutional perversions. Moreover, as if the momentous political events of our times were not enough, events within the intellectual community have further undermined the viability of positive liberty. Whereas Berlin feared that the rational self was endangered by collective prescriptions for its mastery, postmodernists have declared this self an outright fiction. The notion of a fixed rationality that typically grounded the search for a mastered self has become thoroughly suspect. Rationalities may exist but not *a* rationality that transcends culture, gender, history and power. In the postmodern world, the rational self has been deconstructed. The pursuit of positive liberty is thus set adrift without anchor. But the postmodern world has left negative liberty in equally dire straits. As Michel Foucault best demonstrates, contemporary power does not limit itself to prohibiting the fulfillment of desires: it engages in the stimulation and production of desire itself. Power, Foucault insists, should no longer be viewed solely as a juridical and repressive force, symbolized by a king who constrains the movements of his subjects. Increasingly, *biopower* manifests itself as an insidiously creative force, evidenced in the social and political apparatuses and technological networks that shape lives (see Thiele 1986). The postmodern worry is that both the individual and the social body have become the vehicles and chief effects of power—objects of stimulation, manipulation, and production. Power today is in the business of creating subjects, not merely constraining them.

Berlin affirms that negative liberty entails an actual political or public space within which unconstrained thought and action may occur. Freedom is not simply the ability to do as one wishes—a faulty conclusion Berlin attributes to John Stuart Mill. Berlin reasons that if freedom is a function only of desire and not of (external) empirical possibility, then any contraction of desire becomes ipso facto an increase in freedom. His worry is that tyrants could suppress us to the extent that certain desires would atrophy. Under the desire-dependent definition of negative liberty, we would then be considered free(r) because we would feel ourselves to free(r), expressing little or no inclination for abolished political spaces, opportunities, and rights. Freedom—Berlin might have paraphrased Rosa Luxemburg—is truly freedom only if it allows one actually to think, speak, or act differently. The point is well taken. Any political freedom worth its name must allow for the thinking and saying—and to a lesser extent the doing—of what contests the popular and the prescribed. But the postmodern world presents a more complex picture, which, while not obviating the perennial concern of linking liberty to opportunity, casts any straightforward relationship in doubt. Not only the repression of desire but its manipulation and hyperproduction are increasingly in evidence today. The image Berlin supplies to illustrate *his* point is that of the ascetic philosopher-slave Epictetus, who mistakenly imagines himself free because he has learned severely to restrict his wants. If we are to hark back to first-century Rome for illustrations of contemporary threats to liberty, however, then the debauched Nero perhaps better serves our purpose. A surfeit of desire and seduction that militates against meaningful community and politics better limns the dangers of postmodern (Western) patterns of unrestrained production and media-driven consumption. Our subjectification in a technological world—the administration of our identities as desirers, producers, and consumers—is insufficiently addressed by the vocabulary of negative liberty.

How, then, are we to formulate the question of liberty in the postmodern world, supplanting or supplementing the negative liberty which allows personal control over a private domain? Michel Foucault calls for resistance to the productive forces of power that manipulate and produce our identities. Freedom today, he maintains, is to be discovered in the ongoing struggle against the techniques of subjectification. Foucault insists that freedom is less a thing to be secured, like the individual rights and opportunities of Berlin's negative liberty, than an activity to be engaged. "Liberty," Foucault insists, "is a practice," it is "what must be exercised" (1984, 245). With this in mind, Foucault preferred not to speak of an "essential freedom" to be safeguarded so much as an "agonism" inherent in all social and political relations (1983, 222). For Rousseau (and to a certain extent for Cicero, Montesquieu, Locke, and Kant), freedom entailed subordination to a law of one's own making. Foucault adopts the Nietzschean premise that the self, not solely the law, must be of one's own making. With the self no longer given (as the gift of God, nature, or history), freedom is manifest only through its "invention." Hence, we are admonished "to create ourselves as a work of art" (1980, 10–11; 1983, 237). Foucault, however, insists that freedom of the creative self arises only in the social and political arenas wherein our identities become the spoils of battle. His genealogical efforts to destabilize and contest modern modes of subjectification are consequently aimed at giving "new impetus, as far and wide as possible, to the undefined work of freedom" (1984, 46). Effectively, Foucault politicizes Nietzsche's philosophic struggle of self-invention.

Foucault's understanding of freedom bears features of both negative and positive liberty. Foucault adopts the "exercise concept" of positive liberty, wherein freedom is found not in the absence of external hindrances (the "opportunity concept" of negative liberty) but in an activity itself.[4] For Foucault, however, this activity is not tantamount to the actualization of a true or higher self. Like the negative libertarians, he resists defending the nature of freedom or its practice as a function of the individual's completed (rational, moral, or political) development. Foucaultian freedom entails the struggle to remain free from definitions of the self that prescribe its telos. Rather, freedom is found in the construction of a protean self as it evolves by way of the dynamic clash of productive power and resistance. Let us call this Foucaultian (and Nietzschean) understanding of freedom the *postmodern* concept of liberty. In contrast to the freedom *from* (constraints) of negative liberty and the freedom *to* (master a rationally or collectively defined self) of positive liberty, postmodern liberty might be thought of as a freedom *in*. Primarily, this freedom is actualized *in* the struggle of self-creation. As a student of Nietzsche's tragic school of thought, Foucault celebrates this freedom in the seemingly futile and endless attempt to create a self that necessarily remains forever enmeshed in webs of power. Hence, Foucault calls for a "hyper- and pessimistic activism" (1983,232). Postmodern liberty, by this count, is found in the heroic endeavor to create a self as a work of art, or it is not found at all.

Commenting on Berlin's thesis, Alan Ryan notes, "The simplest characterization of positive libertarians is that they identify freedom with control" (1979, 5). Ryan's remark is apt, for positive liberty refers to (one's higher self's) control of one's desires, needs, and (ultimately) of one's world. What Ryan fails to observe, however, is that negative libertarians are also animated by a pursuit of control. For negative liberty, we recall, identifies the individual's control of his immediate environment, of his private life and space. The key difference is that for negative libertarians, sovereignty is limited to a putatively personal realm of objects and relations. While positive libertarians advocate mastery of a transcendent self through collectively mediated, self-given law, negative libertarians advocate mastery of an empirical self over a private domain. According to Foucault and postmodern thinkers in general, the transcendent self is too mythical and the empirical self too malleable for positive and negative concepts of liberty to cut much ice—hence the need for self-invention, a creative resistance to the productive power that envelopes us. But herein, I would argue, postmodern liberty displays its greatest affinity to positive and negative liberty by way of its own, rather subtle pursuit of control.

The postmodern liberation from teleological ends is achieved only by way of the subject's reinvestment in an ideal of artistic mastery. The mastery of the ungrounded but creative self over its own constitution now becomes the Sisyphean task. This move is perhaps best understood as the postmodern radicalization of the self-mastery sought by the Stoic philosophers, whom Berlin correctly identifies as epitomizing positive liberty and whom Foucault chooses to focus upon in his last works. Stoic self-mastery entails the disciplined sculpting of the self into an impervious image of natural law. Postmodern libertarians dismiss natural law (along with gods and transcendent norms). But the nucleus of the Stoic task remains intact. The postmodern attempt to mold the self into a work of art posits the self as a malleable object of production. The task, Foucault states, is to resist the hegemonic "techniques and practices" of power that have constituted the self in the West in order to chart the conditions of an alternative transformation (1993, 224). This autoconstitution of the self is to be achieved by means of the "stylized repetition of acts" (Butler 1990, 140). Fire must be fought with fire. We must become technicians of our own being—dandies of a sort—using our life to fashion ourselves into admirable aesthetic works. Admittedly, Foucault denies that this aesthetic self can gain complete sovereignty within the webs of contemporary biopower. Contingency is everywhere in evidence. Citing Nietzsche, Foucault acknowledges that the individual harbors "not an immortal soul but many mortal ones" and that these are "unable to be mastered by the powers of synthesis" (1984, 94). But, as with Nietzsche, the tragicoheroic effort is made nonetheless. Foucault would have us gain our freedom by moving beyond the "subjected sovereignties" of humanism—beyond "the subject as a pseudosovereign"—to a self whose true sovereignty is marked by the absence of inhibitions in the effort to define its identity (1977, 221–22). In the face of the ubiquitous threat of normalization, the postmodern retaliates with willful self-creation. Will-to-power takes on the persona of the master artist turned upon himself even if, in postmodern times, he is a Picasso, rather than a Rembrandt.

Like William Connolly, I am arguing that "mastery is the route to freedom" for both "individualists" (negative libertarians) and "collectivists" (positive libertarians) (1991, 29). I differ from Connolly, however, in proposing a residual pursuit of mastery, albeit aestheticized, among the Nietzschean and Foucaultian theorists of freedom.[5] Foucault, like most postmoderns, acknowledges his debt to Heidegger, who is claimed as the "essential philosopher" determining his "entire philosophical development." At the same time, Foucault recognizes that Nietzsche "outweighed" Heidegger in influence (1985, 8–9). My argument is that Foucault's understanding of freedom—like his work in general—evidences the burden of Nietzsche (see Thiele 1990, 1991). Despite the differences between positive liberty, negative liberty, and postmodern liberty (and there are many important differences indeed), the identification of freedom with mastery remains central to all. Moreover, whether the mastery in question is of the higher self over the lower self and its desires and needs (as with positive liberty); or the mastery of the empirically demonstrable self over its private domain (as with negative liberty); or the mastery of the aesthetic–agonistic self over its contested, protean constitution (as with postmodern liberty)—each form of liberty tends to loose its quest for mastery upon the world.

FREEDOM—NOT METAPHYSICAL

During his politically active career as rector of Freiburg University under the Nazi regime, Heidegger adopted a positive concept of liberty. In line with Nazi ideology, which he fervently propagated during his brief tenure, Heidegger situates the self within a *Volk* that circumscribes and defines its identity. Thus, Heidegger bore out Heine's prediction of German romanticism's turning the nation against liberal culture. Addressing the question of academic freedom after the imposition of the Nazi *Gleichschaltung,* which sought to organize students and faculty according to the *Führerprinzip* in an effort to integrate the university into the party and state, Rector Heidegger applauds the demise of negative liberty and celebrates its positive counterpart: "This will is a true will in that the German student body, through the new Student Law, places itself under the law of its own essence and in this way for the first time determines that essence. To give the law to oneself is the highest freedom. The much celebrated 'academic freedom' is being banished from the German university; for this freedom was not genuine, since it was only negative. It meant primarily freedom from concern, arbitrariness of intentions and inclinations, lack of restraint in what was done and left undone. The concept of the freedom of the German student is now brought back to its truth" (1985b, 475–76). Here positive freedom is found in subservience to a law of one's own making. But it is not the self as an individual who makes the law but the self as a member of an organic people. One wills the general will as it grows out of a folk and is given voice through the party and the Führer.

After his rectorship, Heidegger would scale back and eventually abandon his advocacy of a (folkish) positive liberty without, however, moving in the direction of negative liberty. He rejects both the latter notion of liberty as "unfettered arbitrariness" and the former notion of liberty as "the constraint of mere laws," including self-given laws (1977b, 25). Heidegger develops a new concept of freedom that effectively extends and reworks his prerectorial approach. By the early forties, he would write: "Freedom is not what common sense is content to let pass under that name: the random ability to do as we please, to go this way or that in our choice. Freedom is not licence in what we do or do not do. Nor, on the other hand, is freedom a mere readiness to do something requisite and necessary and thus in a sense 'actual'. Over and above all this ('negative' and 'positive' freedom) freedom is a participation in the revealment of what-is-as-such" (1949, 307). In important respects, this understanding of freedom resembles Nietzsche's and Foucault's. Heidegger now posits freedom as an activity, event, or happening. Importantly, however, he rejects the postmodern notion of liberty as the mastery of a creative, artistic self. Rather, freedom is proposed as a disclosive "letting-be." I shall argue that Heidegger's development of this *disclosive freedom* presents us with more than just another philosophical profile of a concept already overburdened with meanings. Heidegger's mature understanding of freedom is radically distinct from its modern, metaphysical forerunners and their postmodern derivations; for it offers dignity in a freedom that celebrates guardianship rather than mastery.

Let us begin with a rehearsal of the key features of Heidegger's understanding of human being *(Dasein).* In distinction to all other beings, human being is interrogatively occupied with its own existence. Existing (as an ontic or factual category) is something humans share with all beings. Only human being, however, is ontologically disposed to its life, orienting itself in questioning to its own being. In questioning the Being of its own being, human being cannot help but question that which serves as the ground for all being, for beings as a whole. In short, Heidegger understands human being as that for which Being itself, the Being of beings (das Sein des Seienden), comes into question. He writes in *Being and Time:* "Dasein is an entity which does not just occur among other entities. Rather it is ontically distinguished by the fact that, in its very Being, that Being is an *issue* for it . . . *Understanding of Being is itself a definite characteristic of Dasein's Being.* Dasein is ontically distinctive in that it *is* ontological" (1962, 32). The distinctiveness of human being is not self-consciousness but consciousness of a self that questions its own ground, and ipso facto questions the ground of being in general.

Metaphysics, beginning with Plato's theory of the forms, projects Being as an enduring and unchanging thing. As such it may be represented, by an idea, in our minds. Rejecting this metaphysical orientation, Heidegger proposes Being as nonrepresentable. It is tentatively defined as unconcealment or presencing. We never actually perceive unconcealment or presencing. What is directly available to human being are beings, that which is present-at-hand and ready-to-hand. The actual presencing of that which is present remains hidden and is made available only by ontological questioning. The essence of human being, then, is the provision of a place for Being to become manifest as the questionable and hidden ground of all that is. Heidegger adds that the unconcealedness of beings is a "happening" (1971, 54). Human being is the site of this happening. *Dasein,* we remember, literally means "there-being." Human being is the there where the happening of the disclosure of Being (as nothingness) occurs. In this sense, human being may be posited (to employ Heidegger's locution) as the "abode" of Being (1981, 244). Perhaps better, human being is an abode in *preparation* for Being, an "openness." In human openness, Being becomes manifest—not as some newly discovered thing that lies before us (as a object) or within us (as an idea or form) but as the mystery of its own hiddenness within beings. Because Being reveals itself only through human being, Heidegger dubs human being the *persona,* or "mask," of Being (1968, 62). Only through its mask is Being's presencing discerned. Hence, within the openness of human being, Being shows itself as masked. Outside the openness of human being, Being does not come to presence (as masked) at all.

In distinction to all metaphysical theorizing, Heidegger insists that Being is not something primarily of substance or form. Being is not a thing at all, but the no-thing—"the pure 'Other' than what-is"—that grounds all things (Heidegger 1949, 360). Even if we could rip away the mask of Being, therefore, no discernible features would be revealed lying beneath; for Being is nothing extant. Yet it is precisely the disclosure of Being's no-thingness or hiddenness that human being assumes as its essential task. Heidegger explains by way of a visual metaphor: "Everyday opinion sees in the shadow only the lack of light, if not light's complete denial. In truth, however, the shadow is a manifest, though impenetrable, testimony to the concealed emitting of light. In keeping with this concept of shadow, we experience the incalculable as that which, withdrawn from representation, is nevertheless manifest in whatever is, pointing to Being, which remains concealed" (1977b, 154). As I shall argue, freedom is also experienced as that which, though nonrepresentable, is nonetheless manifest in our capacity to disclose the hidden Being of beings.

Shadows offer the necessary contrast and background for that which is lit. What remains in concealment allows the unconcealed its distinctiveness. It follows that the illuminated opening of human being is largely defined by its shadows. Heidegger understands the perimeter marking the boundaries of our disclosive capacities to be intrinsic to human being and human freedom. Dissociating his from a common understanding of freedom as unrestricted movement, he writes: "It is misguided to think one understands freedom most purely in its essence if one isolates it as a free-floating arbitrariness. Moreover, the task is precisely the reverse, to conceive freedom in its finitude and to see that, by proving boundedness, one has neither impaired freedom nor curtailed its essence" (Heidegger 1984, 196). Every act of freedom is a foreclosing of alternatives and possibilities. Freedom, therefore, is not an unbounded and ungrounded power to do, move, or create. Rather, freedom is freedom to reveal *what is*. But what is, most fundamentally, comes to presence only insofar as it remains illuminated in contrast to what remains hidden. In other words, only the no-thing that is Being allows human being the disclosure of beings or things. In this tensioned disclosure, freedom pertains: "Without the original manifest character of Nothing there is no self-hood and no freedom. . . . Nothing is that which makes the revelation of what-is as such possible for our human existence" (Hiedegger 1949, 339–40). Human being displays its freedom to the extent that it remains open to the inexhaustible mystery of Being in its bounded disclosure of beings. Heidegger writes: "Freedom is the condition of possibility of the disclosure of the Being of beings, of the understanding of Being." (Heidegger 1976–89, 31:303).

Heidegger, particularly in his early writings, referred to the openness of *Dasein* with the term *resoluteness (Entschlossenheit*, lit. "unclosedness"). Heidegger's commentators have often taken his early concern with resoluteness to imply a hard-willed, decisionistic orientation—a Nietzschean or existentialist enamoredness with the power to fashion one's world, where, as Sartre observes, one is "condemned to be free" owing to the burden of total responsibility for the self that one shoulders. Yet the notion of resoluteness, Heidegger at least retrospectively affirms, does not imply Promethean choices. Indeed, it marks the renouncing of such willfulness: "The resoluteness intended in *Being and Time* is not the deliberate action of a subject, but the opening up of human being, out of its captivity in that which is, to the openness of Being" (Heidegger 1971, 67). Hence, Heidegger maintains that "to question is to will to know. He who wills, he who puts his whole existence into a will, *is* resolved. . . . But the essence of resolve lies in the opening, the coming-out-of-cover of human being-there into the clearing of being . . . its relation to being is one of letting-be" (1987, 20–21). The name Heidegger gives to the resolute willing of openness to Being is *Gelassenheit*, a term borrowed from Meister Eckhardt that literally means "letting-be" or "releasement." Human freedom, for Heidegger, particularly after his "turning" of the mid-1930s, is fundamentally and foremost a letting-be.[6]

Openess to Being is effectively an opening of the self to the fundamental interrogation of (its) being. This resolute openness manifests human freedom, for to be occupied with the question of one's own being means "to understand oneself in one's own factual freedom" (Heidegger 1982, 276). This is not to say, however, that human freedom is equivalent to self-absorption. As Heidegger makes clear, we are speaking of the concern for oneself as a human being, that is, as a being-in-the-world-with-others: "To be free is to understand oneself from out of one's own capacity-to-be; but 'oneself' and 'one's own' are not understood individually or egoistically. . . . They are understood in the basic possibilities of transcending Dasein, in the capacity-to-be-with with others, in the capacity-to-be by extant things" (1984, 214). In making its own being an issue, human being opens itself to the question of beings as a whole, which entails opening itself interrogatively to a populated world. Now fundamentally to interrogate this world demands a forgoing of pervasive efforts at its mastery; for fundamental questioning ceases the moment the presumptions and prejudices that necessarily precede such attempts at domination or control arise. To be sure, the disclosure of Being ensues from all human activities and experiences, including human efforts at attaining mastery. But herein Being, as well as the nature of human freedom, becomes obscured. It is revealed only as forgotten. To reveal something as forgotten is to reveal something as no longer in question, as no longer fundamentally questionable. That we are, as Heidegger states, called upon to be the shepherds of Being indicates that we find our freedom not in a forgetful mastery but in a mindful, interrogative caretaking, in concernfully and questioningly letting the Being of beings be. Freedom is therefore most primordially manifest in our care-full being-in-the-world-with-others.

Disclosive freedom, Heidegger insists, is not merely one value among others. Nor either, is freedom a *meta*value, the "value of all values" that allows one to choose between and secure other values (Cranston, 1967, 42). "Freedom," Heidegger writes,

> is not a particular thing among others, not something lined up as part of a row, but rather it prescribes and permeates the totality of beings as a whole. If we are to investigate freedom as the ground of possibility of human being, then its essence is more primordial than man. Man is only a guardian of freedom, . . . human freedom signifies now no longer: freedom as a property of man, but the reverse: *man as a possibility of freedom*. Human freedom is the freedom which invades and sustains man, thereby rendering man possible. (1976–89, 31:134–135; see also 1985a, 9)

Traditional Western thinking posits freedom as the autonomous subject's most valued asset, as its capacity to define and control what it confronts. Heidegger, contrariwise, understands freedom as that which exposes human being to the undefinable and unmasterable, to Being. But what is beyond one's power to master or define is also beyond one's power to calculate and assess. Freedom, therefore, is that unique capacity of human being that allows a reaching beyond calculation and valuation.

Heidegger observes that

> through the characterization of something as "a value" what is so valued is robbed of its worth. That is to say, by the assessment of something as a value what is valued is admitted only as an object of man's estimation. But what

> a thing is in its Being is not exhausted by its being an object. . . . Every valuing, even where it values positively, is a subjectivizing. It does not let beings: be. Rather, valuing lets beings: be valid—solely as the objects of its doing . . . When one proclaims "God" the altogether "highest value," this is a degradation of God's essence. Here, as elsewhere, thinking in values is the greatest blasphemy imaginable against Being. (1976, 228)

Everything Heidegger says here of God or Being might also be said of freedom. Once freedom becomes a value—even the highest value—it ceases to identify that which enables us to partake of the mystery of Being. Subjecting freedom to metaphysical or representational thought—allowing its designation as a value—is an effective assertion of the self's capacity to evaluate and control its relation to Being conceptually if not physically. According to Heidegger, however, "freedom's incomprehensibility consists in the fact that it resists com-prehension . . . because freedom transposes us into the occurrence of Being, not in the mere representation of it" (1985a, 162). Freedom is the gift that allows human being to glance beyond the self—beyond being and its mastery in thought, word, or deed. For Heidegger—and in stark contrast to the Western metaphysical tradition—freedom is not a value, but beyond valuation; freedom is evidenced not in decisionistic willfulness but through careful nonwilling; freedom is not an unbounded power to do but a discovery and disclosing of one's place within bounds.

FREEDOM—POLITICAL

There is no direct, unavoidable and obvious translation of Heidegger's ontological perspective on freedom into a specific political practise. At the same time, our self-understandings as individuals and members of collectivities have important political ramifications. In attempting to answer the question, What shall be done? the discovering and maintaining of a sense of self becomes an essential part of any doing. Our actions in the world stem from, feed into, and flow through our identities. If, then, we accept with Heidegger that "the highest dignity of [man's] essence . . . lies in keeping watch over the unconcealment—and with it, from the first, the concealment—of all coming to presence on this earth" (1977b, 32), it follows that our daily comportment would reflect a certain worldly solicitude. Were modern humanity to discover its dignity in freedom understood as an interrogative disclosure rather than a willful mastery, then significant changes in political culture would be foreseen.

Humanity is now threatened by the ecological limits of a world that it has unceasingly sought to master. I am suggesting that the dangerous self-confidence expressed in humanity's technological exploitation of itself and its world is at least partly explained by the fact that human freedom has been consistently equated with the power to control. Freedom continues to be the Western world's most cherished value. Any change on that score seems unlikely. The political relevance of Heidegger's philosophy is that it allows us to maintain our love of freedom while reining in its more pernicious effects. My argument is not that we must reject out of hand hard-won and still insufficiently propagated positive and negative liberties nor that we must completely shun the aestheticized liberty of postmodern times. But we do need to recognize the significance of disclosive freedom. The task, in other words, is not to bury the past and its achievements but to recognize their dangers while preparing to receive the future. And our future as a species appears doubtful indeed if human dignity can be experienced in nothing but mastery.

Still, the Heideggerian alternative may appear all too passive for many. For does not disclosive freedom reduce us to impotent observers of fate? Is it not simply a recipe for lassitude at the end of humanity's turbulent history of ingenuity, creativity, and destruction? If so, then such freedom would be (as the ballad says) "just another word for nothing left to lose" and nothing to be done. Saint Augustine declared it a great liberty to be able not to sin and a greater liberty to be unable to sin. Heidegger believed that there is indeed much freedom in being able not to will. But he insists that there is no freedom gained in being unable to will. To escape willful mastery, a resolute will is still necessary. Great effort is required (to prepare the self) to let beings be. Just as freedom in resoluteness is not arbitrary willfulness, so freedom in letting-be is not a doing nothing. Disclosive freedom is always the freedom resolutely to will openness to Being. And this openness does not preclude but rather invites human activity and intellect. In turn, letting-be is not tantamount to abandonment of the world or isolation of the self. Quite the opposite: it entails the formation of dynamic worldly relationships, relationships all the more dynamic because they are no longer constrained by the limitations of a subject/object dichotomy.

Heidegger writes: "the freedom to reveal something overt lets whatever 'is' at the moment *be* what it is. Freedom reveals itself as the 'letting-be' of what-is. The phrase we are now using, namely the 'letting-be' of what-is, does not, however, refer to indifference and neglect, but to the very opposite of them. To let something be is in fact to have something to do with it. . . . To let what-is *be* what it is means participating in something overt and its overtness" (1949, 305–6). "To free," Heidegger observes elsewhere, "really means to spare. The sparing itself consists not only in the fact that we do not harm the one whom we spare. Real sparing is something *positive* and takes place when we leave something beforehand in its own nature, when we return it specifically to its being, when we 'free' it in the real sense of the word into a preserve of peace" (1971, 149). The practice of freedom, then, is not the passivity of the fatalist. Indeed, Heidegger distinguishes between fate *(Schicksal)* and destiny *(Geschick)*. Fate compels us to the "inevitableness of an unalterable course"; but nothing in this world, not even the dominance of modern technology, is a fate for human being (Heidegger 1977b, 25). Rather, the historical disclosure of Being, including its current form as technological enframing *(Gestell)*, is a destining. Destiny does not compel, it calls. Human freedom is evidenced in the listening and responding to the destining of Being, which calls us actively to participate in revealing. There is no question of blind obedience, nor is resignation our appointed lot. And the reason, Heidegger states explicitly, is that we are free.[7] The experience of beings in their Being, which for Heidegger delimits the realm of human freedom, should frame our disposition neither pessimistically nor optimistically. Most fundamentally, it yields a tragic experience, that is to say, an experience of resolute action tempered by the wise recognition of limits (Heidegger 1975, 44).

The problem, then, is not the diminution of our creative forces as a species. The problem is that historically these forces have far too often been exercised in the struggle for mastery and domination. This, I believe, is the proper response to critics who interpret Heidegger to promote a sort of political quietism. The typical question is, "How can we think of democracy without imputing to man the minimal will and mastery that Heidegger denies him because will and mastery in some sense already contain the seeds of the world of technology conceived of as the "will to will?" (Ferry and Renaut 1990, 17). The answer is straightforward: Heidegger does not deny human being a "minimal" will and mastery, but he does seek to constrain its unbridled hegemony in the modern world.

The explicitly political nature of Heidegger's understanding of freedom (to repeat) is not unequivocal. One might say that it is political by default, in that its antimetaphysical nature leaves it no better characterization. Alternatively, its political import might best be demonstrated by its capacity meaningfully (though not univocally) to address current political concerns, such as the need for environmental care and technological restraint, the dangerous prerogatives of national sovereignty, the distinctions between public and private concerns, and the development of a nonpatriarchal society. I shall attempt to illustrate my point by critically examining some of the support Heidegger's notion of disclosive freedom finds—and gives—to more explicitly political theorizing.

Disclosive Freedom and Ecological Politics

Freedom in the Western tradition has been identified since Adam with dominion over the earth, and there is little in positive and negative liberty (or its postmodern counterpart) that militates against this. Historically, positive libertarians have celebrated the domination of nature as a testimony to the prerogatives of human rationality. Like the domination of the lower self (or of "inferior" peoples and classes), the domination of the earth often serves positive libertarians as an attestation of human freedom. Negative libertarians, in turn, have fostered such domination in no less degree owing to their preoccupation with the exclusive personal rights of property. Nature, incapable of bearing rights of its own, relinquishes all claim to protection, respect, or care to the extent it is not accorded sanctuary under the aegis of private property. But private property—understood in a Lockean sense as the extension of the self('s labor)—is to be disposed of as its owner sees fit, unconstrained by anything save perhaps the obligation of efficient exploitation. (Thus Locke would justify the expropiation of native American lands without recompense owing to the natives' inadequate exploitation of them. According to Locke, leaving lands unexploited was equivalent to wasting them. And in wasting their lands, the natives forfeited all rights of ownership and defense [Locke 1960, 336–7; see also Glausser 1990].) Postmodern liberty has been around too short a time to have earned any historical indictment on ecological grounds. Yet my sense is that postmodern liberty, which valorizes the powers of artistic creation and heroic struggle, is more likely to depreciate than celebrate qualities of guardianship. The justifiable postmodern concern with the panoptic social administration of selves has a tendency to foster a radically individualistic (even narcissistic) reaction. With the primary postmodern worry being the dangerous encroachments of "pastoral power," any linking of freedom to stewardship would prove problematic (Foucault 1979, 1981). In short, without an understanding of liberty divorced from the will to mastery, the postmodern struggle against subjectification is as unlikely as negative or positive liberty to result in a worldly caring.

Heidegger's philosophy of freedom, on the other hand, buttresses ecological concerns at large. Michael Zimmerman writes that

> Heidegger's critique of anthropocentric humanism, his call for humanity to learn to "let things be," his notion that humanity is involved in a "play" or "dance" with earth, sky, and gods, his meditation of the possibility of an authentic mode of "dwelling" on the earth, his complaint that industrial technology is laying waste to the earth, his emphasis on the importance of local place and "homeland," his claim that humanity should guard and preserve things, instead of dominating them—all these aspects of Heidegger's thought help to support the claim that he is a major deep ecological theorist. (1990, 242–43)[8]

There is much to what Zimmerman says here, and one might add to his list Heidegger's understanding of *Dasein* as care and his definition of human being as the "shepherd of Being." However, one must be wary of straightforward translations of Heideggerian philosophy into ecological practice. For human being, Heidegger insists, is not the shepherd of beings (i.e., the guardian of natural life) but the shepherd of Being. Such guardianship, Heidegger explicitly states, "is not fixated upon something existent" (1971, 184). In turn, while Heidegger does suggest that truth *(Wahrheit)* is to be experienced as preservation and that "preservation belongs to the herdsman," he is also quick to point out that this understanding has "little to do with bucolic idylls and Nature mysticism"; for being a herdsman is only possible if one "continues to hold the place of nothingness." Quite out of keeping with romanticized visions of a seamless pastoral life, the shepherding of Being places one anxiously on the abyss of groundlessness. In short, Heidegger's is a ontological, not a naturalistic, project. A questioning relation to Being may indeed erupt from a solicitous wonder at the vast diversity and beauty of a natural ecosystem. But it may also result from gazing at stars, studying quantum physics, or raising a child in the inner city.

Heidegger, as Zimmerman notes, supports a nonanthropocentric approach to the earth and world. This is absolutely true and has obvious ecological merit. But Heidegger does not suggest the replacement of anthropocentrism with biocentrism—a belief, held by deep ecologists, that relegates the human species the same status as all other organisms.[9] Despite his fervent attack on subjectivism and humanism, Heidegger firmly maintains human exceptionalism on account of the human capacity for freedom: "The question of Being and its variations and possibilities is at heart the correctly understood question of man. Compared with the duration of cosmic galaxies, human existence and its history is certainly quite fleeting, only a 'moment'. But this transiency is nevertheless the highest mode of Being when it becomes an existing out of and towards freedom" (1984, 18). Zimmerman hedges his praise of Heidegger's ecological credentials at this point, citing this passage as indication that Heidegger "overestimated our importance" compared to that of the rest of nature (1983, 121). But Zimmerman overlooks Heidegger's statement a few paragraphs earlier that such fundamental questioning is "far removed from any noisy self-importance concerning the life of one's own soul or that of others" (1984, 16–17). As a being that experiences freedom through its participation in the disclosure of all that is, human being does gain special status in Heidegger's universe. For the disclosive capacities of human beings are, so far as we know, unique: "It is man, open toward Being, who alone lets Being arrive as presence" (Heidegger 1969, 31–32; see also 1962, 28, 35). But Heidegger was explicit that the ontological priority and worthiness of human being in no way determines that the natural world—or the material world at large—exists (solely) for our benefit.[10] Indeed, disclosive freedom flourishes only in the absence of such a possessive, egoistic, and utilitarian assumption. Human being is the highest being only to the extent that human being gains release from all self-aggrandizing subjectivism. If there is a claim of greatness to our being, then it arises not primarily from our mastery of the earth but from the unique human capacity to dwell and disclose in a way that preserves.

Along these lines, one might say that Heidegger supports what today would be called a *bioregional* approach to environmental care. He proposes that intimate ties to soil and locality serve as the antidote for the large-scale centralized manipulation and extortion of nature. Heidegger envisions living and working with, rather than against, natural processes. Hence he contrasts high-technology agribusiness to the work of the peasant, which "does not challenge the soil of the field. In the sowing of the grain [he] places the seed in the keeping of the forces of growth and watches over its increase" (Heidegger 1977b, 15). Unfortunately, Heidegger tends to romanticize the peasantry. He ignores the fact that small-scale farming may be every bit as intent on the unsustainable exploitation of land as large-scale operations. If the peasant proves less ecologically destructive, it may be owing only to the relatively small size of his enterprise. Indeed, to the extent that poverty forces peasants to cultivate smaller and smaller plots of land more intensively, environmental devastation looms. In any case, neither peasants nor business people are born with specifically ecological dispositions. Care for the land—and for people—must be fostered culturally. Heidegger is right in thinking that this care is fostered much better within small farming communities than within large-scale agribusiness bent on ungoverned technological growth. Still, we had best eschew the bucolic romanticism that Heidegger found irresistible; for the rootedness in soil that Heidegger longed for has a seamy political side. In its thrall one might, like Heidegger, succumb to a dangerous infatuation with the power of blood and soil that neatly aligns itself with fascism. In turn, agrarian provincialism may militate against environmental oversight on a global scale. Yet many of the most pressing environmental threats today remain impervious to anything but a globally regulated redress. In any case, the global management of certain ecological concerns does not necessarily disrupt our caring for community or bioregion. Indeed, the environmental slogan Think Globally, Act Locally is based on the general compatibility—even synergy—of global and local concerns. There are no eternal demarcations of one's place of dwelling. Rootedeness, for some, will pertain to a valley or watershed; for some, to a city or nation; and for some, to the earth itself. Heidegger's own preferences for Swabian peasant culture need not constrict our own habitat.

In concluding our critical exploration of Heidegger's ecological credentials, we must acknowledge that our relation to the earth will always entail preservative as well as exploitative and destructive measures. At a minimum, we learn from Heidegger that a balance must be reestablished—a balance most obviously and most dangerously lost in the modern world. Ecological concerns have erupted in contemporary times largely as a result of the increasingly obvious limits to human growth. In effect, Heidegger offers us a philosophy of limits. He describes our freedom as dependent upon rather than curtailed by our worldly boundaries and horizons. With this in mind, disclosive freedom may well provide the necessary corrective to the frenzied technological activity that threatens the earth with its unbounded scope.

Sovereignty and the Politics of Public and Private

In one of her most difficult essays, "What Is Freedom?" Hannah Arendt takes issue with traditional Western notions of liberty (1968, chap. 4). For Arendt, the nature of freedom has been obscured owing to prevailing philosophical concepts structuring the political realm. These concepts identify freedom as a function of (the mastery of) will. In contrast, Arendt observes the freedom is far from being the (will)power to achieve or control one's desires. She indicates (in all probability to the reader's confusion) that action, to be truly free, "must be free from motive on one side [and] from its intended goal as a predictable effect on the other" (p. 151). In direct opposition to the common notion of freedom as the opportunity to fulfill one's wants, Arendt strips from freedom both the final attainment of a goal *and* the initial motivations for its pursuit. The reason is that freedom, for Arendt, is less an individual achievement than a public event. Freedom is manifest in the open spaces of the political realm where the actions of citizens intersect. But action and interaction (as opposed to behavior) are inherently unstable and unpredictable. What humans discover when they act in concert is that freedom is manifest in the very novelty of the results. Far from being controllable and predictable, action, Arendt states, is closer to the nature of a miracle. In action, by way of freedom, the new and the unforeseen occurs.

The pursuit of sovereignty, which orients most Western political thinking about the individual and the state, is identified with the effort to achieve control over the outcome of action. Far from being the prerequisite for freedom, Arendt finds that sovereignty signals its demise:

> Under human conditions, which are determined by the fact that not man but men live on the earth, freedom and sovereignty are so little identical that they cannot even exist simultaneously. Where men wish to be sovereign, as individuals or as organized groups, they must submit to the oppression of the will, be this the individual will with which I force myself, or the "general will" of an organized group. If men wish to be free, it is precisely sovereignty they must renounce. (1968, 165)

Arendt's discussion of freedom is nothing if not shocking for the Western mind. I believe it is most fruitfully viewed as a political reconstruction of Heidegger's notion of disclosive freedom. Heidegger, like Arendt, indicates that freedom evaporates once it is possessed by a sovereign power. Indeed, freedom is not freedom until it possesses us. Like Arendt's conception, disclosive freedom is inherently antipodal to sovereignty and inherently unpredictable. It is revealing of the world and of the new.

Perhaps the aspect of Arendt's political thought most criticized is her distinction between the public and the private realms. We recall that for Arendt the public realm is the birthplace of freedom. Only in the political space devoted not to the necessities of existence but rather to the discovery and preservation of freedom is politics to be found and safeguarded. Freedom therefore lies in stark and unbridgeable contrast to the continuing and unavoidable processes that sustain life in the darkness of privacy. Arendt claims that the divorce between freedom and necessity—the public and the private—is irreconcilable and complete. Anything that issues from life's needs cannot be considered the stuff of politics. The provision of services and goods, the maintenance of households and the exercise of administration, therefore pertains only to the realm of necessity. The public speaking of great words and doing of great deeds maintains a monopoly on freedom. Arendt has presented us with a stunning vision of politics. The wondrous novelty of birth she celebrates is revived in the heroic human capacity for political beginnings. The consequence of her position, however, is that everything that falls between the novelty of birth and the sporadic explosions of freedom evidenced in great speeches and deeds (i.e. the vast bulk of human life) is relegated to the unredeemable realm of necessity.

In the *1844 Manuscripts,* Marx states that man separates himself from the animals by finding his freedom in production beyond physical need (1978, 76). Arendt, despite her virulent attack on Marx's designation of man as an *animal laborans,* effectively extends this enlightenment position. But freedom is now found not in the realm of surplus production but in what might be called the realm of surplus speech and action. Politics and freedom, for Arendt, are manifest only in that human capacity for novelty and creation of word and deed ungrounded in and distinct from the demands of everyday life. Maintenance and preservation, through labor, administration, and general caretaking, is the sorry if necessary task of the unfree. Arendt, like Foucault, celebrates contingency and dismisses sovereignty in the exercise of freedom. And Arendt, like Foucault, insists on the heroic nature of political life. Her standards of heroism, however, come directly from the Greeks rather than via Nietzsche. Consequently, Arendt's criteria for freedom, in contrast to Foucault's, shift from self-creative to world-creative engagements. Nonetheless, her dismissal of the possibility of a freedom exercised in caretaking is equally unrelenting.

We observe that Heidegger's notion of disclosive freedom philosophically buttresses Arendt's political theorizing. Perhaps more importantly, his understanding may also remedy the shortcomings of Arendt's analysis. Arendt wishes to claim that the divorce between freedom and necessity—the public and the private—is irreconcilable and complete. Anything that speaks to the demanding necessities of life does not partake of freedom and ipso facto cannot be considered the stuff of politics. But as Heidegger demonstrates, freedom is evidenced whenever and wherever we open ourselves to the world of beings. Political freedom, then, is evidenced whenever speech and action disclose the world of beings, including our social being-in-the-world, publicly. There seems little reason to exclude from this category, as Arendt unfortunately does, most aspects of worldly life simply because they pertain to the unheroic realm of social, physical, or economic need. Changing a baby's diapers or the oil in one's car, going to the marketplace or out of town for a holiday, do not in themselves evidence much *political* freedom, to be sure. But the provision of day-care facilities and parental leave from work, the promulgation of laws for the mandatory recycling of motor oil, the regulation of the sales of goods according to health and environmental standards, and the provision and maintenance of transportation infrastructure are important political concerns. What makes something political—in these cases and in general—has less to do with whether it pertains to our (basic) needs than to whether and how these needs are collectively addressed and fulfilled. Politics, in other words, may be in evidence whenever the world of beings and the with-world of human beings remain open to public interrogation and disclosure. Political freedom, then, emerges most fundamentally in the caretaking of this public realm. And such caretaking, accomplished in words and deeds, is both preservative and creative in nature. Arendt teaches us that sovereignty, personal or national in scope, is antagonistic to freedom. In the same vein, Heidegger teaches us that grand words and deeds, if carried out in the willful and forgetful pursuit of mastery, may also preclude freedom's greatest exercise. In today's world, where the caretaking of local and global ecologies becomes an increasingly important task, such a Heideggerian understanding of freedom bears obvious political significance.

Being, Doing, and Gender Identity

The boundary between private and public realms is permeable, and the relationship between private and public realms is very much a political one. With this in mind, feminist theorists have offered sustained critical analyses of the ideological foundations of negative liberty. Liberalism, grounded in the concept of negative liberty, evidences an overriding concern for establishing boundaries between individuals in order to proscribe illegitimate interference. Consequently, liberals are primarily concerned with securing personal independence and rights. Many feminists claim that these concerns belie liberalism's masculinist tendencies, most evident in its neglect of what many describe as the predominantly (though not uniquely) feminine concern with establishing and maintaining relationships and community. Nancy Chodorow, relying on the work of feminist anthropologists, psychoanalysts, and object relations theorists, offers an explanation of this disjunction based on the early socialization of children. A girl's sexuality and sense of self, Chodorow theorizes, is "ascribed" to her through her relationship with her caretaker mother. She comes to understand her identity as expressive of her nature rather than explicitly dependent on a performative act. A boy, on the other hand, grows up in most societies with his same-sex parent much in absence and infrequently in the caretaker's role. His sexuality and sense of self are therefore much less the product of an identification with the nurturing parent. Rather, a boy's identity must be "achieved" in distinction from and perhaps in opposition to—the mother. Achieving a stable male identity becomes a matter of asserting independence from the caretaker mother in a defensive establishing of ego boundaries. Moreover, boys' socialization accentuates the performative character of the sexual act. Thus feminine identity is socialized as a being, Chodorow observes, while male identity, constantly in need of demonstration, is socialized as a doing.

The political ramifications of this psychology are not difficult to foresee. The marked differences in the development of male and female identities in early childhood produce a disjunctive and hierarchical social structure. Male values of independence, competitive achievement and aggressiveness oppose, dominate, and are reinforced by female values of dependence, caretaking, and passivity. Chodorow concludes that "until masculine identity does not depend on men's proving themselves, their *doing* will be a reaction to insecurity rather than a creative exercise of their humanity, and woman's *being*, far from being an easy and positive acceptance of self, will be a resignation to inferiority" (1989, 44). Chodorow's research has stimulated many popular works that question the psychological predispositions structuring our social and political lives (e.g., Gilligan 1982; Belenky et al 1986). Without affirming the suspect generalizations of these efforts, I would submit that their resonance with such a wide audience speaks to the increasing instability of traditional (liberal and patriarchal) understandings of political and social identity. The prevalent notions of (negative) liberty that ground these understandings evidence their limits.

Heidegger's understanding of freedom as a disclosive letting-be informs and buttresses this feminist psychology and political theorizing. In a patriarchal world that narrowly conceives freedom as sovereignty, relations of mastery will continue to delimit human identity and define human dignity. As long as our understanding of freedom is restricted to the exclusion of others from our personal space (or, as with positive liberty, to the mastery of ourselves and our world), then our politics will remain one in which the individual meets the world and the other in a singular struggle for self-assertion. Alternatively, if precisely our relatedness to the world and to others begets our freedom, then the struggle for understanding comes to fore. At this juncture, philosophy and psychology most clearly cross paths, and the cultural and political implications of transformed understandings of freedom and dignity emerge. I am suggesting that patriarchal culture will continue to infect politics to the extent that freedom is understood and exercised solely as a masterful, independent doing rather than a disclosive, interpretive way of being.

The practice of disclosive freedom will not obviate the interminable struggles of identity to which human beings are heir. The point is that these struggles may be waged interpretively, for understanding, as well as antagonistically, for mastery. The struggle for mastery need not entail an appreciation of the situated self, and seldom entails an understanding of the other, except insofar as it enables the other's exploitation.[11] On the other hand, the struggle for understanding typically entails the disclosure of the self *and* the other in the context of a shared world. It follows that disclosive freedom—the medium of this interpretive struggle—entails rather than curtails in its own actualization the participation in freedom of that which lies beyond the self. In these terms, the significance of disclosive freedom for a democratic politics emerges.

CONCLUSION

Twenty-five hundred years ago Pericles admonished the Athenians in his Funeral Oration, "Make up your minds that happiness depends on being free, and freedom depends on being courageous" (Thucydides 1954, 149–50). In fifth-century Athens, freedom already had become the most cherished value. The pursuit of this value, however, quickly became a mask for imperial mastery. The development of Western freedom has evinced numerous variations of this theme. Hegel could write with typical aplomb that "the history of the world is none other than the progress of the consciousness of freedom" (1956, 19). Hegel was not mistaken in his assessment of the tenor of history, for he correctly discerned the historical origins of Western freedom in the dialectic of master and slave.[12] At the end of history, Hegel announces, there is universal acknowledgement not that some are free and others in bondage but that man as such is free. And yet, firmly situated within the Western metaphysical tradition, Hegel could not see beyond its perimeter. Consequently, Hegel maintains that modern freedom is gained only by exchanging the mastery of some over the lives of others for humankind's presumed mastery over history itself.

The Western development of the consciousness of freedom, from its inception, remains in the service of such quests for mastery. Only the historical subjects and objects of domination change—from the domination of the master over the slave in ancient Greece, to that of the higher over the lower self with the Stoics, to Rousseau's (Marx's) romantic collectivization of this ideal in the reign of the general will (proletarian consciousness) over the individual, to liberalism's claim of the individual's sovereign control over a private realm, to postmodern mastery wherein the existence (let alone maintenance) of any such borders to the self are denied, while the ironic attempt is made to transform self-control into a full-fledged self-creation. I am proposing that we ponder a freedom no longer wedded to mastery—*mastery* being defined as willful control of the world, the other, a private domain, or the self (be that self understood as transcendental but repressed or awaiting its protean constitution).

Negative liberty is apt to degenerate into license and the breakdown of community; positive liberty, into tyranny and totalitarianism; postmodern liberty, into apathy and despair in the face of a ubiquitous web of power or (alternatively) into the isolated self-aggrandizement of one who in the face of inescapable normalization nihilistically embraces an unbounded self-invention. Disclosive freedom, no doubt, has its own pathologies and susceptibilities. Openness to the mystery of Being might degenerate into fatalism, the reining in of will might deteriorate into passivity before the powers that be. I am not proposing, then, that disclosive freedom wholly substitute for negative, positive, and postmodern liberties. Rather, the task is one of problematizing, supplementing, and perhaps sublimating their practise. Just as Berlin believes that positive liberty (as self-government) should be viewed primarily as a means to protect negative liberty, so one might argue conversely that negative, positive, and postmodern liberties should be viewed as necessary means to protect and foster the conditions favorable to the exercise of disclosive freedom. And what Berlin concedes of positive liberty I would unbegrudgingly affirm for negative and postmodern liberties, as well: their exercise easily leads to dangerous abuses and perversions; yet held in balance they remain "essential for a decent existence."[13] Heidegger, unfortunately, was reluctant to make such concessions. Still, he does not completely impugn the practical worth of prevalent notions of freedom. Rather, he observes their limitations and notes specifically regarding negative and positive liberty and their Nietzschean alternative that disclosive freedom stands over and above them from a philosophic perspective.

I have insufficiently indicated the political implications of disclosive freedom. My sense is that its political implications are necessarily ambiguous. This ambiguity is, to be sure, unsettling. But we should not be dissuaded from the exploration and practise of disclosive freedom for that reason alone. Isaiah Berlin, in closing his essay on the two concepts of liberty, asserts that the principle underlying negative liberty—perhaps only the "late fruit of our declining capitalist civilization"—is no less valid because the duration of its historical ascendency can not be guaranteed (1969, 172). The "relative validity" of our convictions, Berlin insists, should not dissuade us from upholding them. Indeed, he maintains that it would be a sign of a dangerous political immaturity to demur in the face of such uncertainty. I submit that the decaying fruits of our ecologically declining civilization have germinated the seeds of a disclosive freedom. This freedom, properly cultivated, offers us dignity and stamina in the political struggle against the "unchecked momentum" and "irresistible power" of a technologically driven way of life. Whether disclosive freedom will find a foothold in the postmodern world and, if so, just what its historical role and longevity might be surely remains beyond our power to determine. But, like Berlin, I would argue that the convictions and experiences that underlie it, philosophical and worldly in nature, are on this count no less valid.

Notes

This paper was originally delivered at the 1992 American Political Science Association Annual Meeting in Chicago. I would like to thank my discussant, Stephen K. White, as well as Fred Dallmayr and other audience members for their comments. Funding for this paper was provided by the Social Science and Humanities Research Council of Canada.

1. I am not completely alone in this belief. Fred Dallmayr also finds that Heidegger's thought "from the earliest to the latest writings is permeated by the endeavor to grapple with the category of 'freedom'—in a manner that decisively moves beyond the confines of traditional liberalism and libertarianism" (1984, 207; see also Haar 1989).

2. Berlin notes that in theory negative liberty might have similar psychological requirements, demanding knowledge of which self is not to be interfered with and constrained. Historically, however, this has seldom been the case. Those promoting negative liberty have simply identified the empirically demonstrative self as the true self. Hobbes offers the most straightforward example of this understanding: "Liberty, or Freedome, signifieth (properly) the absence of Opposition; (by Opposition, I mean externall Impediments of motion;) and may be applyed no lesse to Irrational, and Inanimate creatures, than to Rationall" (1968, 261–62).

3. Even John Stuart Mill, Berlin notes, is guilty on this count. Berlin's views on this issue have been hotly disputed. Many have straightforwardly concluded against him that "to give up, for the sake of justice some freedom to act is not to diminish liberty" (Reed 1980;376).

4. I take this characterization of Berlin's two concepts from Taylor 1979.

5. Connolly also worries that Heidegger facilitates his being taken prisoner by the "communitarian" camp, who see freedom as an "attunement" to the world (1991, 225). Heidegger's talk of attunement *(Gestimmtsein)*, however, refers not to faith in the "higher direction" of nature or the cosmos but (as we shall see) to an appreciation of profound mystery.

6. Heidegger's turning, or *Kehre*, signals his shift from a focus on the heroic capacity of human being to disclose Being to a focus on human being's participatory relationship to Being's disclosure. In an insightful examination, Michel Haar (1989) suggests that human freedom becomes enveloped and overpowered by the "narcissism of Being" burgeoning in Heidegger's later writings. Haar's concern is based on the assumed equivalence of human autonomy and human freedom. However, once we accept that disclosive freedom is better described in terms of contextual participation than in terms of sovereign possession, Haar's worry about the "dispossession" of freedom by Being appears misplaced.

7. Jerry Weinberger (1992), like many others who are polemically engaged with Heidegger, mistakenly assumes that the technological stamping of the age is equivalent to a pessimistic technological determinism. Heidegger, however, turns the tables on those who make such charges. He writes, "All attempts to reckon existing reality morphologically, psychologically, in terms of decline and loss, in terms of fate, catastrophe, and destruction, are merely technological behavior" (Heidegger 1977b, 48).

8. In the same vein, Charles Taylor writes that "Heidegger's understanding of language, its *telos*, and the human essence can be the basis of an ecological politics, founded on something deeper than an instrumental calculation of the conditions of our survival (though that itself ought to be enough to alarm us). It can be the basis of in one sense a 'deep' ecology" (1992, 266).

9. Heidegger is frequently misinterpreted on this score (see, e.g., Devall and Sessions 1985, 99). A literal biocentrism, if conceivable in practice, would certainly be undesirable. The AIDS virus and the useful bacteria of our digestive tracts are most interesting specimens of life. But that we should cease to exploit the latter or combat the former seems to me as silly as it is *unnatural*. Nature, as Nietzsche was fond of reminding us, is all about exploitation and battles to the death, and we should not be arguing for a world in which viruses have the same "rights" to life and growth as children (see Thiele n.d.).

10. We read, for example: "That all functional relations are grounded ontologically in a for-the-sake-of in no way decides whether, ontically, all beings are as beings for the sake of the human Dasein" (Heidegger 1982, 295).

11. The best account of this dynamic that I am aware of is Todorov 1984.

12. One might argue that the Western valorization of freedom is primarily the product of slavery, that the value of freedom has developed out of the desire to escape the master's power. Hegel and Nietzsche both make this point in different ways with a respective historical and psychological intent. Recently (and with a sociological slant), Orlando Patterson has cogently reformulated the argument that "freedom was generated from the experience of slavery" (1991, xiii).

13. In a recent interview, Berlin states:

> The only reason for which I have been suspected of defending negative liberty against positive and saying that it is more civilized is that I do think that the concept of positive liberty, which is of course essential to a decent existence, has been more often abused or perverted than that of negative liberty. . . . Certainly the weak must be protected against the strong. . . . Negative liberty must be curtailed if positive liberty is to be sufficiently realized; there must be a balance between the two, about which no clear principles can be enunciated.

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American Political Science Review Vol. 88, No. 2 June 1994

POLITICS AND THE ENVIRONMENT: NONLINEAR INSTABILITIES DOMINATE

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This analysis investigates a formal nonlinear systems model characterizing longitudinal change in the environment as a function of oscillating partisan control of the White House. It is assumed that one political party will tend to favor help for the environment despite some economic costs, whereas the other party will generally favor economic growth over environmental concerns. These policy changes affect the environment interactively with both public concern for particular environmental problems and the economic costs relating to environmental repair. This interaction with policy changes causes a disruption in the continuously evolving balance between the social factors that damage the environment and the environment's own ability to recover. The disruptive potential to the environment is considerably ameliorated with a reduction in the electoral cycling.

The relationship between presidential elections in the United States and the degradation of the environment is not thoroughly understood. In this analysis, the relationship is characterized as a nonlinear interaction between oscillating environmental policy positions due to changes in partisan control of the White House and two other critical inputs. These inputs are public concern for the environment and the economic costs of environmental cleanup.

Broadly, this addresses a contextual question relating to political structure. The current theoretical connection between the dual cycles of partisan control of the White House and public concern for certain issues finds direct correspondence with recent research on cycles in the public mood by Stimson (1991). The joint effect of electoral structure and political context on this cycling is an extension of a thematic approach to the study of political parties as pursued by Beck (1974), Huckfeldt (1983), and Sprague (1981), in which party activities are seen as agents influenced by the structure of various social constraints. These constraints, combined with normal party activities, often produce nonobvious by-products. I investigate this phenomenon here with regard to environmental policies. The results of the current investigation suggest that the interaction of the inputs just mentioned can lead to a surprisingly high potential for significant, and perhaps dangerous, volatility in the quality of the environment.

The relationship between democratic political structures and the environment is a relatively new area of research. It is easy to see how a rigidly communist or dictatorial society could inflict substantial environmental damage locally (or, in some instances, even globally), since public pressure for such governments to be environmentally conscientious is typically lacking.[1] But environmental damage occurs in democratic societies as well, and the level of this damage can be severe indeed, despite ostensibly "popular checks" against blatant abuse.

During the past 20 years, there has been an increasing global awareness of the immediacy of our planetary environmental worries. In part, this is evidenced by the existence of international meetings dealing with the environment, such as the June 1992 meeting in Rio de Janeiro of the United Nations Conference on Environment and Development. However, what has become increasingly obvious in international negotiations relating to the environment is that the dominant consideration of each of the participants is the likely impact on their own domestic politics. It is not well understood how the organizational structure of each society's politics may influence longitudinal change in national environmental policies and thus long-term change in environmental degradation. In short, understanding the domestic aspects of environmental policies goes well beyond obtaining recent survey data of the general public's attitudes. If serious damage is being done to the environment, understanding how governmental structure, particularly democratic political structure, contributes to the outcome is critical to our being able to minimize that damage in the future. The focus here is on an aspect of the electoral structure of presidential politics in the United States as an example of this relationship between democratic political structures and environmental damage.

Crucial to this discussion is the understanding that in democratic societies in general (and certainly in the United States), environmentalism is subject to the same fate of political trade-offs as is any other concern. The typical environmental trade-off is economic, as measured in terms of losses in gross domestic product or jobs. Indeed, this is the focus of a recent analysis of the economic consequences of global warming by Schelling (1992). In one respect, the current investigation is an attempt to extend the discussions between environmentalists and economists to a broader range of social scientists as well, including political scientists who are interested in democratic electoral politics.

This analysis proceeds by developing a model of change in environmental degradation that is structured by electoral change, public mood, and economic cost constraints. Simulations are then performed on the model that reveal its more basic dynamic properties by systematically changing one input at a time while holding all others constant. An analysis of some of the global properties of the model is then conducted over a continuous and realistically wide range of potential parameter values important to the process. Substantive conclusions are then drawn from this characterization of the political–environmental process.

THE EXTENSION FROM ECONOMICS TO POLITICS

Most of the social science literature on environmental degradation points to economic consequences of particular environmental change (e.g., global warming, ozone destruction, toxic contamination), including governmental responses to these changes. Particularly far-reaching in this regard is Schelling's (1992) analysis of global warming. (Other prominent examples of the environmental–economic connection include Darmstadter 1991, Moulton and Richards 1990, Nordhaus 1991a, 1991b, and Poterba 1991).

On a more political level, one focus of the extant literature has been on the bureaucratic or regulatory responses to competing political and environmental demands. Tobin's (1990) analysis of the regulatory failure associated with biological diversity is seminal in this regard. Also, Wood and Waterman have produced two pioneering pieces of research that seek to identify some of the political determinants of longitudinal changes in environmental regulatory practices (Wood 1988; Wood and Waterman 1991).

A difficulty in studying the interaction between politics and the environment is the current lack of data with regard to long-term environmental degradation. However, we are not at a total loss in this regard. Environmental modelers routinely develop models in the absence of data to explore the consequences of environmental changes to a broad range of interests, including ecosystem stability (R. May 1974), global warming (Energy Modeling Forum 1992), and other aspects of planetary transformation (see *New York Times,* Nat. Ed., 5 May 1992, pp. B5–7). In such cases, models are developed in anticipation of the data that are sure to arrive eventually.

In the current absence of data, model simulations are used to understand the environmental consequences of human action. Indeed, the current separate worries regarding ozone holes and global warming are a response to just such model simulations. In part, simulations are useful because a society may want to *avoid* a particular consequence of human activity. Thus, data for catastrophic scenarios can, hopefully, never be collected if the simulations lead to policy changes that avoid the catastrophes. In fact, simulations of plausible models are the *only* path available to us to understand the consequences of a given course of action without actually performing the action and waiting to see if disaster strikes in, say, a hundred years.

Yet models and simulations are built upon an understanding of the basic inputs of a process. Fortunately, we know a great deal about democratic electoral processes in the United States. Relevant to the current investigation, we know how constituent pressures are aligned differentially with regard to the political parties and how governmental policies reflect these constituent pressures. Moreover, in the social sciences more broadly, there exists a long and rich tradition of exploiting models similar to the one developed here (with or without data) using simulations. This includes areas of research as diverse as political economy (Hibbs 1977), contextual theories of voter activity (Brown 1991; Huckfeldt 1983; Przeworski and Soares 1971; Przeworski and Sprague 1986), arms races (Richardson 1960; Ward 1984), and racial segregation (Schelling 1978). Thus we have a methodological tradition as well as sufficient substantive knowledge to begin investigating an area of research that we might call "political ecology."

THE MODEL

In constructing a model of longitudinal change in environmental degradation that will be heuristically useful for analysis with simulations, it is wise to follow two basic guidelines.[2] First, the model should be general. Thus, model complexities should be held to a minimum so that it is relatively easy to identify change in the model behavior caused by varying each input. Second, it should be easy to identify the type of input that each of the components of the model contributes. In this case, the types of inputs will be limited in a classical fashion to gains and losses.

We begin by constructing a model describing change in environmental damage. Beginning with the gains (i.e., increases in degradation), a straightforward approach is to assume that environmental damage will increase logistically. At first, pollution will increase exponentially as industries grow and populations consume more products that harm the environment. However, this cannot continue forever, even in the complete absence of environmental legislation. In a worst-case scenario, people would eventually die, perhaps of starvation if the polar ozone holes spread to temperate zones, leading to diminished agricultural production due to the higher levels of ultraviolet radiation. But in a less draconian fashion, one would expect that environmental damage has some upper limit beyond which a society will not willingly go. To ease the discussion of the model, the level of environmental damage, X, will be scaled to have an upper limit of unity. Thus, increases in environmental damage can be expressed as

$$dX/dt = rX(1 - X), \tag{1}$$

where r is a constant parameter of the model and represents what we can label as the "pollution growth rate" parameter.

Some readers may wonder whether it is correct to model environmental degradation as a smooth growth process, since it may seem as if occasional spurts of activity would make the "ride" more bumpy. As with all models, the model developed here contains some simplifications, and a degree of "smoothing" is a desired trait of all attempts at segregating systematic and stochastic components of longitudinal change. However, many spurts in environmental degradation may be accounted for by the complicated oscillatory components of the model in its more fully developed form.

It is necessary to include two separate loss terms in equation 1. First, governmental environmental policy can act to limit or repair environmental degradation. This occurs, for example, when governments clean up toxic waste dumps, prohibit lead in gasoline, or (perhaps in the future) orbit large mylar balloons to reflect some amount of solar radiation in the event of catastrophic global warming (see Schelling 1992). The amount of governmentally inspired reduction in environmental damage is likely to depend on the interaction between the amount of damage that exists, and the current level of public concern for that damage. Such connection of policy to longitudinal cycling in the public mood can be deduced from empirical work by Stimson (1991), MacKuen (1981), and others and has played a notable role in Rowland, Lee, and Goetze's (1990) theoretical specifications involving cycling catastrophe models of environmental change. The theoretical justification for a specification for this loss is explained more thoroughly below. The second loss term to include with equation 1 addresses the ability of the environment to repair itself over time. Pollutants tend to decay, be they chlorofluorocarbons or toxic wastes. Some have short half-lives, while others stay around considerably longer, but virtually all eventually decay.

Thus, we can now write the complete expression of change in environmental damage as

$$dX/dt = rX(1 - X) - pXY - kX. \qquad \textbf{(2)}$$

Here, the variable Y is the current level of public concern for environmental damage, and the parameter p identifies the effectiveness of governmental policies in reducing current levels of damage as an interactive function of the level of damage and public concern for the environment. The parameter k is a decay parameter that reduces environmental damage based on some proportion of current levels of that damage. In this analysis, k is set equal to the expression, $-\ln(.5)$/halflife, where halflife is the number of years before one-half of the damage decays by itself.[3] This allows us to examine the model based on how long a particular pollutant is expected to stay around once it is released into the environment. For immediate purposes, the term halflife can be thought of as an average half-life for current and total environmental pollutants. Note that the entire expression in equation 2 limits the level of environmental damage to the range of zero to unity. This convenience further generalizes the model to an acceptable range suitable for numerical investigation.

The second major input into the environmental relationship identified is public concern for the environment. Again, we will have gains and losses in public concern. As with environmental damage, it is reasonable to place a logistically defined upper limit on public concern. In this case, public concern for the environment would increase as damage to the environment increases up to some limit (call it the "panic" limit).

However, the public does not typically react to current levels of environmental damage in an instantaneous fashion. There is usually a lag in public concern as people wait until the environmental damage begins to affect them directly. Since the direct effects of environmental damage are usually due to pollutants introduced into the environment some time previously, the public is actually responding to an earlier level of environmental damage. Indeed, this lag can be as short as a few years or as long as many decades, depending on the particular type of environmental damage being considered. For example, it took a few decades for environmental pressure to build substantially with regard to the poisoning of the Great Lakes, yet concern for the ozone holes and global warming increased more quickly once the connections between these phenomena and human behavior were identified. For modeling purposes, we can say that there will be an average lag in public concern. Thus, public concern will increase as a function of previous levels of environmental damage up to some limit. As with environmental change in equation 2, it is convenient to set this limit at unity.

Decreases in public concern for the environment are most likely to be due to the costs of cleanup. If people are going to have to pay substantially higher gasoline taxes or taxes of another type, concern to clean up the environment is likely to diminish, on average. Thus the total expression for change in public concern for the environment can be written as

$$dY/dt = X_{\text{old}}(1 - Y) - Z. \qquad \textbf{(3)}$$

Here X_{old} is the lagged value of the level of environmental damage, and Z is the cost associated with cleaning up the environment. Growth in public concern for the environment continues as long as concern is not yet near its limit and costs are relatively low. Once concern is near its limit or costs are high, costs will tend to dominate the dynamics in equation 3 and concern for the environment will begin to diminish.

The costs of cleaning up the environment will vary as well. There will be gain and loss characteristics of the change in costs. With regard to the gains, costs will tend to increase as *both* concern for the environment and actual environmental damage rise. Concern for the environment will spur politicians to address environmental issues, but politicians will allocate money for the environment only when there are clearly definable problems that can be addressed. Yet there are limits to the funds available for all governmental projects. For numerical purposes, we can scale the variable for costs such that the upper limit for governmental spending on the environment is unity. Thus, spending for the environment will increase in the direction of this limit as long as there is increasing public concern for the environment and sufficient current environmental damage.

Longitudinal decreases in governmental spending on the environment are likely to be due to the magnitude of the current burden of environmental spending. Environmental spending will tend to increase as long as costs are low. As costs increase (and especially when costs approach their limit), public concern for the environment will tend to diminish (perhaps some environmental problems may appear resolved), and overall spending will tend to decline. Thus, the equation describing longitudinal change in spending for the environment can be expressed as

$$dZ/dt = XY(1 - Z) - Z. \qquad (4)$$

In combination, equations 2–4 constitute a nonlinear system of three interconnected differential equations. The three state variables—environmental damage, public concern for the environment, and spending for the environment—interact longitudinally in a continuous fashion. As the system is currently specified, interactive cycling among all of the state variables is possible, as I shall demonstrate. However, to capture a more complete characterization of the cycling between public concern for the environment and electoral activity (a generalized empirical reality described more fully in Stimson 1991), this system requires a modification.

The modification is to allow the system to vary according to the partisan politics originating from the White House. This modification can be accomplished using parameter p in equation 2. The analysis has two levels of sophistication. As a first approximation to modeling the interaction between politics and the environment, parameter p oscillates between two different values, depending on the ideological perspective of the current president. When a conservative president occupies the White House, that president's supporters are not likely to include those environmentalists who want large increases in governmental spending on the environment, and thus parameter p is likely to have a relatively low value. However, a more liberal president will be more closely tied to the desires of environmentally active constituents. In such a situation, the value of parameter p is likely to be relatively high. Thus we will need to vary the value of parameter p in a systematic fashion in order to see how the change in presidential perspective affects overall environmental damage, given the other elements in the model.[4]

Oscillating the value of the parameter p between two discrete values is considered a first approximation to modeling the interaction between politics and the environment because many environmental policy changes that occur across administrations may happen more gradually. Discrete changes are examined first in order to identify causally the behavioral characteristics associated with specific components of the model in its simplest form. Later in this analysis, the model is extended to include gradual changes in the parameter p that would reflect less sudden policy alterations that may be more typical of many situations.

But let us be clear at the outset about what is being captured by the parameter p. There are two ways of thinking about the parameter. The first is that the government actually cleans up the environmental mess that it and others made. However, a second motivation underlying parameter p reveals a more subtle reasoning.[5] Governments sometimes do clean up toxic disasters. But, perhaps more commonly, governments actively reduce environmental degradation by phasing out dirty technology in favor of newer and cleaner technology. Examples of this can be found in the construction of power plants, the use of catalytic converters on cars, the removal of lead from paint, and many other areas. Industry initially resists such conversions to cleaner technology, but once the conversion is made, the situation is relatively permanent. The reason for the permanence is cost. Industry does not want to invest in new infrastructure twice—once to clean things up and a second time (if allowed) to get dirty again.

The desire of industry to stay clean once it is forced to invest in cleaner technology does not nullify the reasoning behind the specification using parameter p. The reason is that society is always changing, and industry is always growing. Thus, there will constantly be new products and new industries that will be producing new sources of environmental degradation. The parameter p captures the government's overall ability to keep on top of this ever-expanding and constantly changing problem. It is not just that government sometimes cleans up environmental damage. Government also inhibits, through a variety of mechanisms, the development of new environmental problems. Thus, a high value for the parameter p reflects a government that is actively engaged in developing and maintaining a cleaner environment through a variety of means. The phasing out of old, dirty technology and the gradual introduction of cleaner technology is just one such means. The parameter p captures the summary effect of the total efforts.

There is very substantial empirical evidence suggesting that varying the value of the parameter p (both discretely and gradually) is a useful approach to modeling the interaction between politics and the environment. Since public concern for the environment began to heighten in the 1970s, there have been marked contrasts across administrations with regard to environmental concerns. Wood and Waterman have demonstrated that agency leadership is the critical factor in determining the direction of administrative environmental activities (Wood 1988; Wood and Waterman 1991). This affects dramatic and sudden shifts in agency funding and contributes to the long-term effectiveness of regulatory activity. Indeed, political appointments have greater influence over environmental activity than legislation, budget variations, or even actions by Congress. In sum, administratively orchestrated politics determines whether or not an environmental agency will be an effective advocate of environmental concerns.

Two heuristic examples are useful here. In the early Reagan years, a determined (and generally successful) effort was made to curtail quickly the scope, enforcement, budget, and effectiveness of environmental regulation. (For the detailed story of these efforts, which included the very effective appointment of an antienvironmentalist attorney, Ann Burford [née Gorsuch], see Harris and Milkis 1989; Vig and Kraft 1984; Waterman 1989; Wood 1988; and Wood and Waterman 1991.) However, such visible efforts are not needed in order to subdue an agency. Late in the Bush administration, it was reported that the Justice Department systematically but quietly blocked the prosecution of environmental crimes, thereby fatally derailing the efforts of the Environmental Protection Agency to enforce its own regulatory activity (*New York Times*, Nat. Ed., 30 October 1992, p. A13).

Thus, there is no one way to measure an administration's concern or lack of concern for the environment. If an administration wants to undermine environmental regulatory activity, there are many ways to do it. Moreover, the same is true if an administration seeks to be broadly supportive of environmental concerns. It is for this reason that parameter p is specified as a summary estimate of all current environmentally related activity that reverses total levels of degradation, assuming that each administration will use whatever means are currently at its disposal to achieve its desired goals.

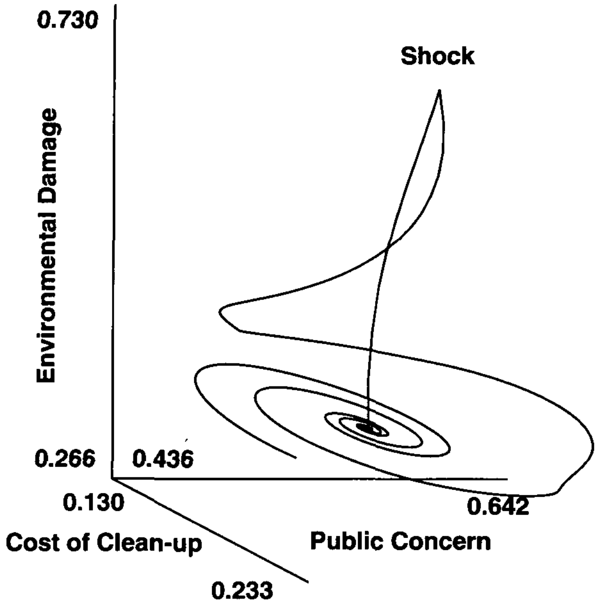
THE NUMERICAL EXPERIMENTS

As is typical of nearly all such nonlinear and interconnected systems of equations, it is not possible to uncouple the separate state variables in order to evaluate the system's behavior using techniques of indefinite integration. Moreover, linearizing techniques are both cumbersome and not very useful in the current situation. Rather, to learn from the system outlined, it is best to approach the system numerically with Hamming's maxim constantly in mind: "The purpose of computing is insight, not numbers" (Hamming 1973, 3).

The strategy of the current approach to analyzing the system is similar to the way one would approach any empirical problem. First, a baseline description of the system is established. From this baseline description, the general behavior of the system in its simplest form is identified. Subsequently, experiments are performed in which one change is made to the system at a time, thereby gaining an understanding of the effect of that one influence on the overall model.[6] In this way, the model in its baseline characterization acts as the controlling specification with regard to the component that is being varied.

FIGURE 1

Phase-Portrait Representation of Environmental Damage, Public Concern, and Environmental Cleanup Costs: Sample Trajectory with Six-Year Public Lag and *No* Partisan Policy Differences



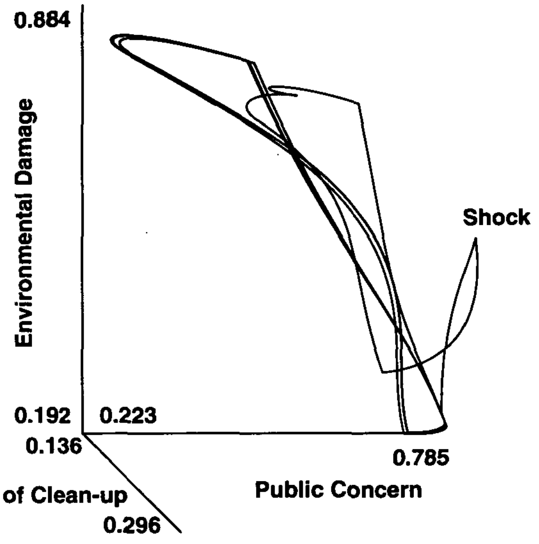
Note: All trajectories spiral to a single global attractor, thus moderating environmental damage, despite short-term catastrophes.

Figure 1 presents a phase portrait representation of a sample trajectory for the model.[7] This figure is used here as the baseline characterization of the system's behavior. The three axes represent the three state variables in the system: environmental damage, public concern for the environment, and environmental cleanup costs. In Figure 1, the parameter p is held constant with a value of 1 and is therefore not varied between two values.[8] This represents a situation in which there are no policy differences between the parties and all presidents behave similarly with regard to favorably promoting environmental policies. Moreover, the lag in public awareness with regard to concern for the environment is held at a moderate value of 6 years for this simulation.

Note that the trajectory spirals into a stable equilibrium point from its initial condition. Moreover, this equilibrium point reflects a relatively low level of overall environmental damage, as would be expected in a situation in which both parties favorably support environmental policies. After the trajectory's arrival in the proximity of the equilibrium point, a large shock is added to the system to see how quickly the trajectory would return to the neighborhood of its equilibrium. This shock represents what would happen to the system following a short-term ecological disaster. Note that the return trip is relatively rapid.

FIGURE 2

System Phase Portrait: Sample Trajectory with Six-Year Public Lag *and* Partisan Policy Differences



Note: With a public lag of six years and political party differences, a stable orbit develops that leads to large—and potentially disastrous—changes in environmental damage, as well as cleanup costs.

Thus, the system as represented in Figure 1 (i.e., in the absence of partisan policy differences and with a moderate lag in the public's perception of environmental damage), supports (at equilibrium) a stable low level of environmental damage with moderate levels of public concern and relatively low financial costs. The swings encountered while arriving at that point may seem a bit rough, particularly following a major shock to the environment. But the eventual arrival at a pleasant and sustainable equilibrium does not take long. All this makes sense and would be expected from a properly functioning representation of this process under the given conditions.

Partisan differences in environmental policies are added to the system in Figure 2, that being the only difference. Thus, in Figure 2, the parameter p was allowed to oscillate between 0 and 1 every eight years. This represents a situation in which each party would hold substantial differences in environmental policies and the two parties would rotate control of the White House every eight years.[9] A value of 0 for parameter p would reflect an administration that, say, strongly supports economic growth over environmental protection. Thus, governmental actions to decrease further damage to the environment would be minimal at best. A value of 1 for parameter p would reflect an administration that actively promoted proenvironment policies.

In the situation given in Figure 2, note that a large stable orbit develops in the system. Moreover, even the ecological shock to the system disturbs the trajectory from this stable orbit only briefly. Thus, in the long run, the model is quite sensitive to environmental policy variations between the parties that rotate control of the White House.

From an environmental and cost point of view, the model's behavior as represented in Figure 2 is not good news. The problem is the large magnitude of the cycling and the consequent high level of governmental financial costs. When, say, a pro-jobs president is elected, the environmental policies of his or her predecessor may have reduced the level of environmental damage. Moreover, with environmental damage more or less under control, the public's concern for the environment can begin to diminish. This leads to a relaxation of proenvironmental legislation and regulatory activity with its consequent eventual increase in environmental damage. At this point, the public desires change, a new party occupies the White House, and the process of reducing damage to the environment repeats.

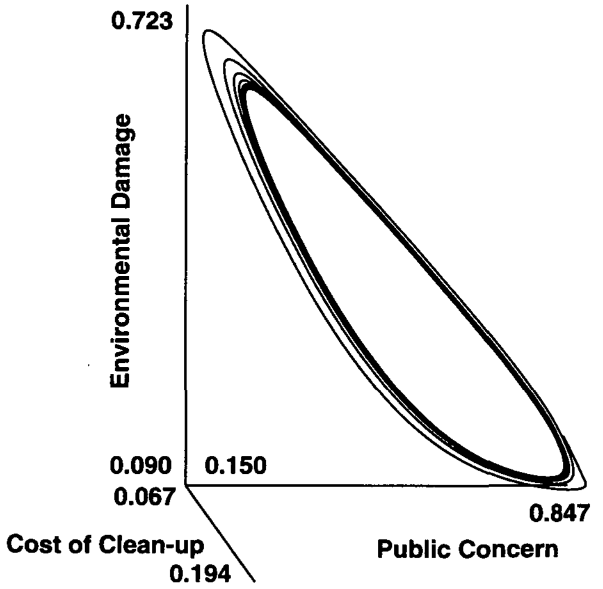
One major problem identifiable in Figure 2 is that the overall costs of this cycle appear to be much greater than the relatively moderate and stable costs represented in Figure 1. From an environmental point of view, the repeated and high levels of damage are the problem. But from a financial point of view, the cycle itself, with its large and costly swings into the expensive programs needed for environmental recovery, is the most damaging. Indeed, from both points of view, it might seem a better choice to reduce such long-term costs as well as environmental damage by avoiding the cycling.

But differing party policies with regard to the environment is not the only thing that can cause large magnitude and costly swings with this system. Indeed, even in the complete absence of political differences, the system is sensitive to the length of the lag in the public's perception of environmental damage. This sensitivity is demonstrated in Figure 3. The only thing changed is the length of the lag in public concern regarding environmental damage. Policy differences between the political parties are not included in the computation of the trajectory in this figure. Here, the lag between environmental damage (e.g., the release of pollutants) and the public becoming concerned about the damage is 15 years. The result of this increased lag is that the system develops a large stable orbit. This is a substantial characteristic change from the stable equilibrium that dominated the trajectory in Figure 1. It is clear in Figure 3 that the trajectory slowly converges to an orbit in which there are large changes in environmental damage, public concern, and economic costs. This is despite a political situation in which both political parties pursue similar environmental policies![10]

When political policy changes occur together with an increased lag in the public's concern for environmental damage, the oscillations continue, but with more abrupt changes in direction and a much more complicated orbital form. Figure 4 combines both the longer 15-year lag as well as 8-year policy changes in the White House. From the phase-plane perspective, the orbit in Figure 4 seems to "flap" at lower levels of environmental damage, somewhat mimicking the

FIGURE 3

System Phase Portrait: Sample Trajectory with 15-Year Public Lag and *No* Partisan Policy Differences

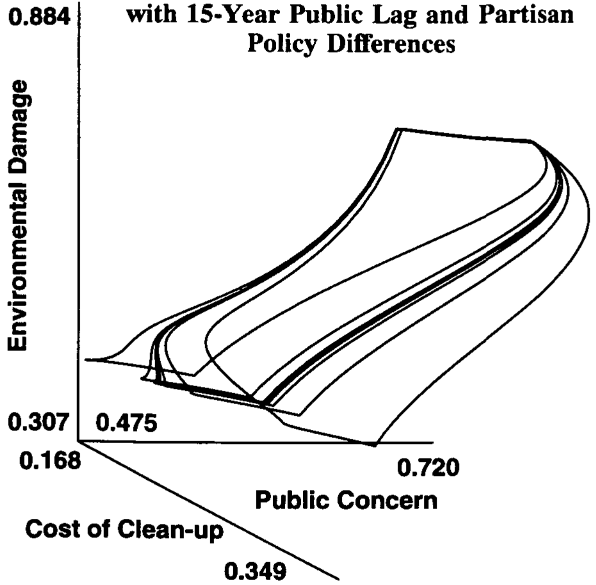


Note: An increased lag in public concern produces large swings in environmental damage, despite no political party policy differences.

swings of a pendulum. From this figure, it is clear that directional changes in environmental damage can be much more radical than those associated with Figure 3. Of course, these changes are associated with substantial changes in costs. But the costs often vary in this figure out of phase with changes in the other variables.

FIGURE 4

System Phase Portrait: Sample Trajectory with 15-Year Public Lag and Partisan Policy Differences



Note: The combination of an increased lag in public concern for environmental damage and political policy changes produces a "flapping" oscillatory orbit.

This temporal disconnectedness between costs and actual damage is directly tied to the interactive influence of the long lag in the public's awareness and the discontinuities inherent with the political policy differences. Moreover, this type of lagged cycling may be common. Notably, such cycling has been observed between the public mood and partisanship by Stimson, using a large body of survey data (1991, 93–94).

In terms of a goal of maintaining a healthy environment, this latter situation could be quite dangerous. The overall impact on the environment is one of rapid departures from previous states and thus an increase in the longitudinal volatility of environmental quality. In some respects, this situation may partially reflect the general characteristics of the contemporary state of much environmental management in the United States today.

GRADUAL POLICY CHANGES

In its present form, the model presented here assumes that governmental policy changes with regard to the environment are sudden. In some—perhaps many—situations, this may not be an unreasonable assumption. Wood and Waterman have offered empirical evidence suggesting that national environmental policy changes are primarily influenced by the nature of top political appointments rather than by legislation, changing budgets, or Congress (Wood 1988; Wood and Waterman 1991). Moreover, these policy leaders can display a remarkable ability to produce a wide range of change very quickly.[11]

However, not all policies—environmental or other—will change suddenly. Indeed, more gradual changes across a broad range of policies may likely be closer to the norm of regulatory politics. At first, this may seem like a good thing. Gradual changes enable governments to come to grips with the consequences of previous policies and thereby to fine-tune subsequent policy developments. Thus we would expect increased predictability and a reduction in environmental instability when policy changes are more gradual. This, in turn, promises a reduction in the overall level of environmental damage. Surprisingly, these intuitions may be largely false. Indeed, gradual changes in policy need not eliminate dangerous and large-scale oscillations in environmental damage, and they can also end up hindering our overall ability to manage the environment.

To show why we are not necessarily better off with gradual policy changes with regard to the environment, it is necessary to reformulate the model to include gradual, rather than sudden, changes. Thus, we need a new equation in the system. This equation will structure change in the parameter p, the government policy response to environmental damage. A high value for p suggests that the party in power is strongly engaged in reducing environmental damage through regulatory and other activities, whereas a low value for p indicates the opposite. Previously, the model has allowed p to flip from one value to another, thereby reflecting discrete changes in policy preferences across administrations.

We now want to vary p continuously, rather than discretely. That is, the value for parameter p must change continuously in the direction of a desired partisan goal for that parameter. For example, a Democratic administration may desire p to have a high value, whereas a Republican administration may prefer it at a lower value. These desired values are partisan goals, to which the value of the parameter p should asymptotically approach, depending on which party is in power at a given point in time. Algebraically, this is accomplished by introducing into the system the equation

$$dp/dt = ep(g_{\mathrm{Dem}} + g_{\mathrm{Rep}} - p). \qquad (5)$$

In equation 5, g_{Dem} and g_{Rep} are the partisan goals for the parameter p. Only one such goal can operate at a time, since only one party at a time can control the presidency. Thus, when a Democratic administration is in power, g_{Dem} is set to its ideal value and g_{Rep} is set to zero, the reverse being true when a Republican administration is in power. Thus, all of the terms in parentheses in equation 5 act in combination as a logistic directional control for the evolving value of parameter p. For example, when a Democratic administration is in power, the value of p changes in the direction of g_{Dem}. Change slows down when p approaches g_{Dem} as a limit. The parameter e identifies the rate (proportional to the current value of p) at which this movement in the value of p takes place. All of this changes p from a constant parameter to a variable parameter. The entire model is now a four-equation system, which is collected and summarized as follows:

$$dX/dt = rX(1 - X) - pXY - kX$$

$$dY/dt = X_{\mathrm{old}}(1 - Y) - Z$$

$$dZ/dt = XY(1 - Z) - Z$$

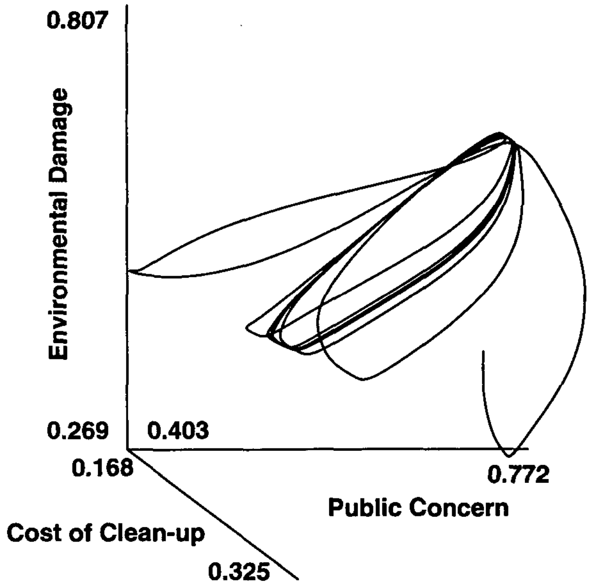
$$dp/dt = ep(g_{\mathrm{Dem}} + g_{\mathrm{Rep}} - p)$$

Again, the variables and parameters are

X	environmental damage
Y	public concern for the environment
Z	economic costs of environmental clean-up
p	governmental policy response to environmental damage (a variable parameter)
g_{Dem}	ideal Democratic policy response goal
g_{Rep}	ideal Republican policy response goal
r	pollution growth rate parameter
k	natural pollution decay rate
e	parameter determining speed of government policy changes toward partisan goals

FIGURE 5

System Phase Portrait: Sample Trajectory with 15-Year Public Lag, Partisan Policy Differences, and Gradual Partisan Policy Changes



Note: The combination of an increased lag in public concern for environmental damage and gradual political party policy changes can produce smoothed versions of the flapping oscillation observed in Figure 4. Here, $r = 1$ and $e = 1$. Democratic and Republican ideal policy response goals are 1 and 0, respectively. This corresponds to position A in Figure 6.

Our first test to see the effect of this change in determining the value of parameter p on the overall behavior of the model will be to duplicate all of the other conditions used to create Figure 4, which included partisan policy differences as well as a 15-year lag in public concern for environmental damage. This new simulation is presented in Figure 5. The values of parameters r and e are set equal to 1, and the Democratic and Republican ideal policy response goals (g_{Dem} and g_{Rep}) are set equal to 1 and 0, respectively. Thus the situation is very similar to that of Figure 4, except that the value of parameter p is allowed to move continuously and gradually between two oscillating limits. From Figure 5, the primary effect of this change on the overall model is to round off the sharp edges of the trajectory that were previously the result of sudden policy changes. Large-magnitude oscillations in environmental damage still occur, and the flapping oscillation pattern of Figure 4 reappears, albeit in the form of rounded loops rather than more sharply angular movements.

A natural observation relating to Figure 5 is that we are looking at only one value for the parameter e. Recall that the value of parameter e controls the rate at which government policy changes toward the partisan goals emanating from the White House. High values for this parameter corresponds to rapid movement toward these goals. Very high values can produce movement that mimics that associated with sudden shifts in policies, as portrayed in Figure 4. Very low values make movement between the two goals sluggish at best.

At this point in the analysis, we need a way to portray the overall behavior of the model through a continuous and realistically suitable range of values for the parameters *e* and *r* without paging through a countless series of trajectory phase portraits. This is important because we do not yet have a hundred or so years of data from which to estimate these parameters. One counterargument to the current analysis would be that the particular values used for these two parameters are not likely to be the exact values that the system will actually have once it is estimated, given an appropriately long collection of time-series data. Other parameter values may produce less dangerous systemic behavior. Moreover, it is likely that these parameter values will migrate over time (i.e., not remain constant) to keep pace with evolutionary changes in our social and political cultures. Thus, it is important to describe the general topology of the environmental playing field on which the values of these parameters are placed so that one can generalize about the overall characteristics of the system. As will become obvious, one can find little solace in the idea that it may be better to wait to collect the data before worrying about the intricacies of the political–environmental connection. Indeed, in terms of environmental risk and a desire to engineer an effective collection of environmental policies, it may be difficult to imagine a less hospitable general setting.

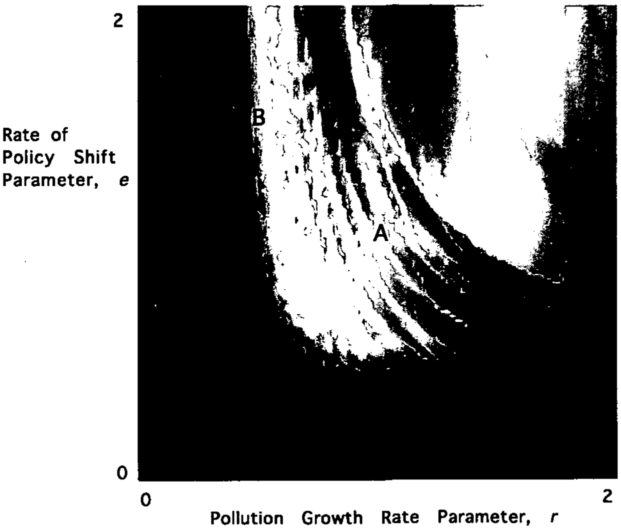
To do this, a new measure is needed that will capture two aspects of the overall behavior of the model. The first is the sum of the absolute value of total change in environmental degradation. Small values of this aspect indicate that change is slight and thus probably more manageable from a regulatory perspective. On the other hand, large values of total change indicate dangerous levels of environmental volatility. The second aspect of our new measure must indicate how much directional change characterizes the model's behavior. This is captured by counting the total number of changes in sign in the model's four derivatives for a fixed period of time.

The new measure is calculated by weighting the total magnitude of change in the environment by the total number of sign changes in the system's derivatives. This measure is an estimate of overall environmental turbulence. High levels of environmental turbulence indicate large magnitudes of overall environmental damage combined with frequent oscillations in the direction of change. Low levels of this measure indicate relative stability in environmental damage, both in terms of the total magnitude of change and the direction of movement. The measure is comparable across trajectories as long as the length of time for all trajectories is held constant.

Figure 6 is a portrait of the measure of environmental turbulence across a continuous range of values from 0 to 2 for the parameters *e* and *r* (the pollution-growth-rate parameter).[12] The environmental turbulence measure is being shown on the third dimension of the figure by the shading. Brighter shades in the portrait indicate high levels of environmental turbulence whereas darker shades indicate lower levels of turbulence. The letter "A" on the figure indicates the location of the parameters used to construct the trajectory presented in Figure 5.

FIGURE 6

Portrait of Environmental Turbulence for the Entire System Based on Continuous Ranges of Potential Values for Two Parameters



Note: This is a portrait of the total magnitude of change in the environment weighted by the number of sign changes in the system's derivatives. Brighter shades indicate large-magnitude changes in the quality of the environment combined with many shifts in the direction of change for the variables (i.e., high levels of environmental turbulence).

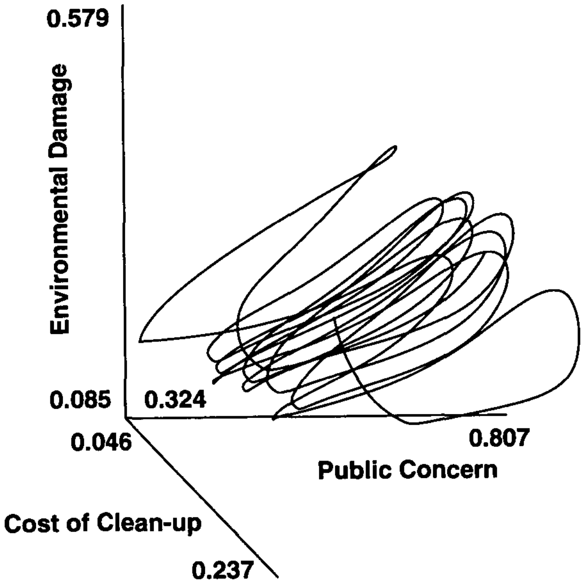
In Figure 6, note that continuous change in the values of the parameters does not produce gradual changes in the measure of environmental turbulence. Indeed, the portrait in Figure 6 is filled with closely interwoven variations in shading, indicating rapidly changing levels of turbulence. That is, turbulence does not increase smoothly as, say, industries increase their level of pollutant discharge. Nor is the change in turbulence gradual if the government pollution reduction rate changes evenly. Moreover, the only areas in Figure 6 in which darker shades dominate in a significantly continuous fashion are for situations with very sluggish rates of partisan policy change (i.e., very low levels for the parameter *e*) or under conditions of very low levels of environmental damage, neither of which seem realistically to describe current regulatory or environmental conditions.

In order to manage the environment, one must be able to predict what will happen when changes are made. The results of Figure 6 suggest that this may not be easy or even possible, given current measurement inadequacies. Attempts to manage the environment within a setting of significant differences in partisan approaches can lead to large-scale and highly volatile environmental changes, many of which may not be easily reversible from an ecological point of view.

As bad as this sounds, the situation is worse. There are large areas in the parameter spaces of parameters *e* and *r* for which the determination of what will happen next in the environmental trajectory is nearly

FIGURE 7

System Phase Portrait: Sample Trajectory with 15-Year Public Lag, Partisan Policy Differences, and Gradual Partisan Policy Changes Using Alternate Parameter Values



Note: Modest parametric variation in the four-equation model can produce very complex oscillations in environmental damage. Here, r = .5 and e = 1.7. Democratic and Republican ideal policy response goals are 1 and 0, respectively. This corresponds to position B in Figure 6.

impossible to predict. This is due to complexities in the oscillatory components imbedded in the trajectory movements. The oscillations portrayed in Figure 5 seem easy to follow, but for other values of the parameters e and r, the situation changes dramatically and can easily become much more complicated.

To show these conditions, an additional phase portrait is constructed, using the parameter values as identified in Figure 6 by the letter *B*. Figure 7 presents such a complex trajectory using the parameter values $e = 1.7$ and $r = .5$. From this figure, it is clear that policy managers would have a difficult time predicting the future of environmental change given this political and pollution context. The oscillations are evident, but they do not settle down into a clearly discernable pattern within any reasonable length of time. On a technical level, such complexity in trajectory structure may not be certifiably chaotic (classically defined), since the characteristic Lyapunov exponents calculated using these parameter values do not indicate a strong sensitivity to initial conditions (analysis not shown). However, a Fourier analysis of the system's periodicity (not shown) indicates a complexity very typical of chaotic systems. From a macro substantive perspective, there seems little difference between these complex oscillations and pseudorandomness, since the level of longitudinal complexity is sufficiently high to make a practical ability to predict the future virtually impossible.

Substantively, this implies that a great deal can happen quickly with regard to the environment when small changes occur in pollution and policy rates and when the political parties differ in their environmental policies. This is precisely the fear raised by Schelling with respect to global warming (1992, 8). It is this potential characteristic of the environmental system that holds the greatest long-term danger for planetary ecological management.

CONCLUSION

It would be absurd for an astronaut in a space vehicle to start a fire in the cabin to keep warm. No one would question this because we understand that the regulatory mechanisms within the vehicle would be overloaded from the consequences of the fire. The problem with making a similar statement with regard to damaging the environment of our planet is that we do not entirely understand the automatic processes of environmental management. These processes include political structural components as well as ecological and physical components, and these political structural components have been largely ignored in the environmentally related literature.

I have endeavored to describe some of the political components that may strongly interact with the rest of the overall biological system. The normative hope is that the discussion will engender a more balanced approach to the discussions of our environmental problems. The social-scientific angle to the environment is not just economic. Since politics is at the root of all attempts at environmental management, political scientists should play a major participatory role in the evaluation of current problems and in the prescription of current and future remedies.

This analysis presents a model of environmental change in which the political, social, and economic inputs into the system interact nonlinearly to produce highly varied global patterns of ecological damage. The model is developed with respect to presidential-electoral and regulatory practices in the United States. The results of the analysis suggest that relatively minor parametric changes in the system can lead to major alterations in the longitudinal patterns of environmental change.

One of the major implications of these experiments is to suggest that the regulation of our environment may be a much more challenging task than we have presently envisioned. Indeed, our current electoral structures may directly assist the long-term degradation of our environment. The complexities that exist within the simplified system investigated here suggest a greater level of complexity in the actual physical system. The political components of that complexity certainly play an important role in the determination of the general structure.

In terms of recommendations, it is likely that substantial benefit for environmental management from a political point of view would be to reduce the impact of party policy differences on ecological cycling. A complete plan on how to do this is beyond my present scope, since I am here concerned more with demonstrating the dilemma we are in than formulating a way out. However, some suggestions to initiate the discussion of solutions may be helpful.

First, in the Federal Reserve Board we have an example of an institution developed in order to deal with potential cycling problems (in this case, in the economic arena). Thus, we have experience with related problems in other areas and the effectiveness of some of the solutions that have been attempted. It is not likely that a simple political compromise between the political parties would last sufficiently long to be meaningful to the long-term global properties of the system. The temptations to exploit short-term political gain would eventually destroy any temporary effects that would result from the compromise. Something more permanent and substantial is needed, and the creation of some type of independent Environmental Preserve Board may be one answer.

It may also be possible—perhaps more so in countries other than the United States—to create, through constitutional means, an elected position independent of the influence of the office of president or prime minister. This elected official would serve as the head of the environmental regulatory agency of a country. This could have major implications on the management of the environment, since the official would gain reelection solely on the basis of whether voters are happy with how the environment is being regulated. Thus, it would not be possible to avoid the issue of the degradation of the environment by talking about other issues in which the voters might also be interested. The basic problem is to isolate the environmental concerns from other political matters.

Yet another suggestion would be to institutionalize the role of the United Nations in monitoring—and to some extent regulating—the planetary environment. Rules could be constructed that would establish planetary norms with which each country would have to abide. Some fair system of penalties would have to be established in order to obtain cooperation across nations. Such penalties would likely involve trade and economic issues, but technology transfers and political cooperation in other areas could also be factors. Interestingly, the United Nations already seems to be becoming much more involved in new global matters since the end of the Cold War. Thus, the stage may already be set for the emergence of a new environmental role for this organization.

But it is important to understand that helpful environmental management cannot function only on the level of bureaucratic organization: individuals must understand the need for this management. In particular, the regulation of the environment is influenced by the lag in the public's perceptions of our environmental problems. To some extent, this can be addressed through an enhanced role for environmental education in our society. On the level of formal education, it is most likely that a comprehensive approach to curriculum reform across all age categories will be required in order to achieve any lasting benefit to the overall problem, since generational biases often persist for lifetimes and it may be too late in the game for college courses to make a meaningful impact.

This last recommendation cuts to the heart of the matter. If nonlinearities dominate the political–environmental system, it may be futile to try to fine-tune current environmental policies in a rational-decision-making sort of way in an attempt to fix the ecological problems. If political leadership, regardless of party, tends (at least on average) to reflect in practice the policy preferences of large numbers of a nation's citizens, then it is necessary to upgrade the preferences of the citizens through education before environmental disaster does it for us. It is not that evil political and corporate leaders continually dupe the ignorant political masses. Rather, in a democracy, it is the masses that let their leaders do what they do. The bottom line is that unless large majorities of the citizens of democratically governed countries become strongly environmentally sensitive, there can be no long-term solution to this problem. The electoral and ecological cycling will continue as parties with differing and large constituencies oscillate in office, and the costs of environmental cleanup will remain both high and volatile. Possible governmental–structural fixes may help in the long run, but only if the educational component is also present.

Fundamentally, any successful approach to environmental management will require an understanding of the political processes that influence that management. At the outset, it is important to understand how these processes extend to the basic electoral practices of our democracy. The current discussion has attempted to demonstrate that the political–environmental connection in the context of contemporary democratic governance is complex. This nonlinear complexity is associated with questions relating to human quality of life, and indeed, survivability. We address these matters not as mathematical curiosities but as issues with a direct bearing on the physical characteristics of our planetary habitat.

Notes

1. Indeed, the collapse of the former communist governments of Eastern Europe and the former Soviet Union led to near-immediate revelations of how deep and widespread such damage can be (*New York Times,* Nat. Ed., 13 May 1992, pp. A1, A4.)

2. See Mesterton-Gibbons 1989 for a more complete overview of such guidelines.

3. In all of the numerical results presented herein, the half-life for the system's environmental damage was fixed at six years.

4. Recent anecdotal evidence nicely illustrates how sitting presidents can and do change regulatory environmental policy to achieve electoral benefit with regard to current constituents. The following list of articles and editorials, all drawn from the National Edition of the *New York Times* during the 1992 presidential primary season, describe how President Bush made an effort to reduce, wave, or eliminate environmental regulations that adversely affected potential or current supporters of his campaign. This partial listing is *not* meant to

imply that President Bush is unique in any way in this regard. The argument made here is that this is a natural occurrence of partisan politics in the United States in which environmental concerns fall victim to the same constituency pressures as any other issue or set of issues.

"Icy Words on Global Warming," 30 March, p. A14
"Administration Tries To Limit Rule Used To Halt Logging of National Forests," 28 April, p. A7
"The Environmental Pollution President," 29 April, p. A15
"Freezing Government," 4 May, p. A14
"2 Admit E.P.A. Violated Hazardous Waste Law," 8 May, p. A10
"E.P.A. Head Allows Project on a Lake Michigan Marsh," 9 May, p. A7
"Showdown on Endangered Species," 11 May, p. A15
"White House and Congress Face Showdown on National Forests," 12 May, p. A8
"Bush to Relax 1990 Rule on Air Pollution Notices," 18 May, p. A9
"Environment Laws Are Eased by Bush as Election Nears," 20 May, pp. A1, A10

5. I am indebted to G. Robert Boynton for suggesting this second justification for parameter *p*.

6. This approach to the numerical evaluation of such systems is described more fully in Kocak 1989, as well as Mesterton-Gibbons 1989.

7. Throughout this analysis, all numerical solutions to the differential equations were obtained using a fourth-order Runge-Kutta algorithm. Moreover, all of the results presented here were obtained using software written by the author in Think Pascal and run on a Macintosh Quadra 900. The computer was made available to the author through a grant administered by James Johnson, vice provost of Emory University's Information Technology Division.

8. In Figures 1–5, the parameter *r* is given a value of 1.

9. Of course, actual partisan changes would not be as evenly spaced. Holding the spacing constant here, however, acts to control the structure of this new input, thereby clearly identifying its effect on the overall system.

10. By way of casual observation, this scenario indeed seems a partial contributor to what may have happened to the Great Lakes of North America. The public's response to the increasing levels of pollution was very slow, probably due to the indirect ways that lake pollution affected peoples' lives.

11. For example, the Bush administration passed a flurry of regulations regarding the environment in the final days of its tenure (*New York Times*, Nat. Ed., 16 January 1993, p. A1). Similarly, the Reagan administration engineered a dramatic decline in funding for the Environmental Protection Agency during President Reagan's first term in office (Wood 1988).

12. Figure 6 was constructed using imaging software provided by the National Center for Supercomputing Applications at the University of Illinois, Urbana-Champaign.

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American Political Science Review Vol. 88, No. 2 June 1994

DIVIDED GOVERNMENT IN THE AMERICAN STATES: A BYPRODUCT OF LEGISLATIVE PROFESSIONALISM?

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Since World War II, divided government has become increasingly common in the American states. A significant component of the increase is the deterioration of Republican fortunes in state legislatures: after the 1990 and 1992 elections, for example, only five state legislatures were controlled by the Republicans. I shall examine the hypothesis that the professionalization of state legislatures makes legislative service more attractive to Democratic candidates and less attractive to Republican candidates, because full-time legislative service is incompatible with another career, and Democrats, on average, have less lucrative career opportunities than Republicans. Statistical analysis of post–World War II legislative elections outside the South is consistent with the argument: other things being equal, every ten-thousand-dollar increase in real biennial legislative compensation is associated with approximately a 1% increase in Democratic legislators.

Bill Clinton's election brought an end to an unprecedented 12 consecutive years of divided national government. But few observers are confident that Clinton's plurality victory has banished divided government from the scene, especially with the Democrats defending two-thirds of the Senate seats to be contested in 1994. Given the frequency and duration of split control of national institutions in recent decades, some have labeled the contemporary period a "new era of coalition government" (Sundquist 1988), while others, continuing to adhere to the electoral realignment–party system interpretation of American electoral history, view the contemporary period as a "sixth party system" (Aldrich and Niemi n.d.; see also Shafer 1991).

Students of elections have increasingly begun to focus on the electoral causes of divided government (Campbell n.d. [a]; Ginsberg and Shefter 1990; Jacobson 1990). The research question typically has taken one of two forms: (1) Given their continuing advantage in party identification, why have the Democrats had so much difficulty constructing presidential majorities? and (2) Given their sweeping presidential victories, why have the Republicans been unable to convert their presidential support into congressional—especially House—majorities? Commentaries adopting the first question as a starting point typically focus on a combination of divisive social issues and disappointing economic and foreign policy performance, perhaps exacerbated by an open nominating process that highlights the divisions within the Democratic party. Commentaries starting with the second question typically focus on Democratic gerrymandering and incumbency, though academic research casts doubt on such explanations (Jacobson 1990; Mann 1987). These two starting points obviously are not mutually exclusive: the Democrats may fall short of their potential in the presidential voting while the Republicans simultaneously fall short in the congressional voting. There is reason to believe that divided national government is a complex rather than simple phenomenon, with multiple causes that may be logically and empirically independent.

Recent research further complicates the discussion. I have noted elsewhere that American state governments have also become increasingly divided (Fiorina 1992; see Figure 1). Moreover, the features identified in state patterns of divided control resemble those in the national case. In particular, the Republicans do somewhat better in executive races than state partisanship would predict, but they have steadily lost ground in state legislatures. These developments suggest the operation of factors broader than those typically used to explain the components of divided national government, for if developments in Ohio, California, and other states resemble those in the nation at large, the existence of common or related causes should be considered, at least as a starting point.

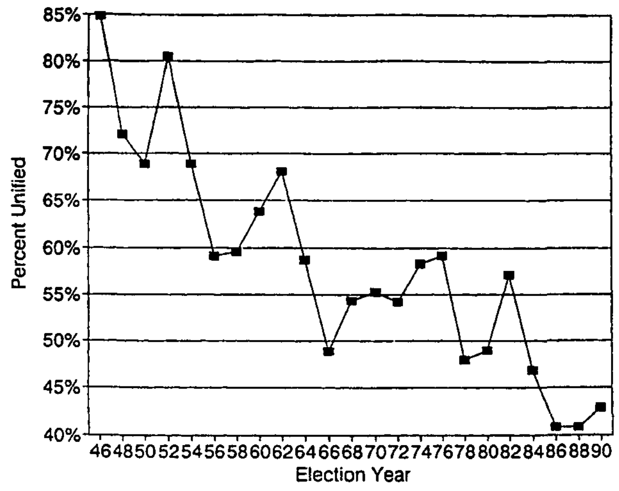
I shall focus on *one* aspect of the growth of divided government in the American states—the decline of Republican fortunes in legislative elections—and *one* contributing factor to this decline, namely, the development of legislative professionalism, or (as some might prefer to call it) careerism. Briefly, the analyses that follow are consistent with the argument that all other things being equal, professionalized legislatures are relatively more attractive to Democrats than to Republicans. Thus, postwar good government reforms that encouraged the professionalization of state legislatures had the unintended consequence of undercutting the Republican party in that arena.[1] Moreover, given that state legislatures are the traditional "farm teams" for Congress, the decline in Republican state legislative fortunes should naturally lead to some decline in their congressional fortunes as well.

PARTY FORTUNES IN STATE LEGISLATIVE ELECTIONS

The decline in unified state government largely reflects a decline in unified *Republican* state govern-

FIGURE 1

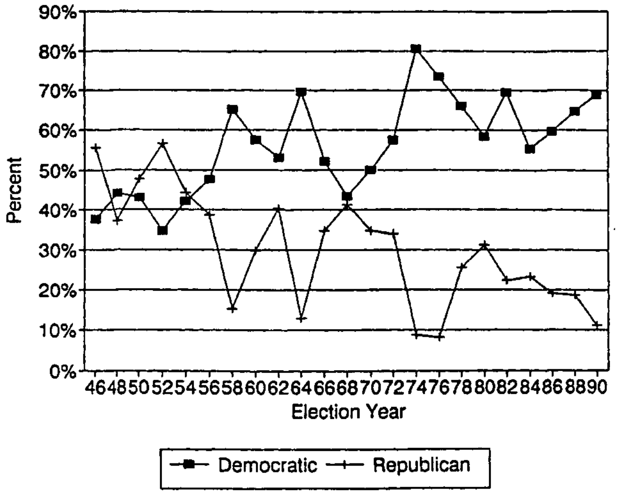
Unified State Government



ment, which in turn largely reflects a decline in Republican *legislatures* (Fiorina 1992, chap. 3). Since World War II, there has been considerable variability but no partisan trend in gubernatorial elections. Legislative elections are another matter, however. Even while spotting the Democrats a 15-state lead in the southern and border states, in 1946 and 1952, the Republicans captured an absolute majority of state legislatures (Figure 2). Their fortunes have slipped dramatically, however, and following the 1990 and 1992 elections, only five states had Republican legislatures. The impact of adverse national circumstances is clear (the 1958 and 1974 recessions, the Goldwater debacle), but it is also clear that such adverse shocks are embedded within a long-term secular decline. Note that the Republican collapse in state legislatures had taken place by the mid-1970s (Figure 2). Over the more recent period spanned by the State Legislative Elections Project (1968–86), there is little variation in the number of Democratic state legislators.

FIGURE 2

Unified Legislatures



Interestingly, although there are several excellent studies of state legislative elections, none of them have addressed the Republican decline. Analyzing elections in 41 states between 1944 and 1984 Campbell (1986) finds that popular presidential candidates help state legislative candidates of their parties and that the longer the presidential coattails, the larger the subsequent off-year loss. Because Campbell's dependent variable is *change* in legislative seats, however, the steady decline in Republican strength does not stand out. Chubb (1988) examines Democratic lower-house success in nonsouthern legislatures over the period 1940–82, finding that state election results are significantly influenced by the coattails of candidates for higher offices, by national economic conditions, and by turnout surge and decline. He does not comment on the declining percentage of Republican legislators, though the decline is statistically incorporated in a lagged variable measuring the percentage of lower-house Democrats in the preceding legislature. Similarly, Simon, Ostrom, and Marra (1991) find that control of legislative chambers between 1950 and 1988 depends on such short-term factors as the president's standing in the polls and dramatic political events and such longer-term factors as the national party balance and the departure of the state outcome from its long-run equilibrium. Again, these authors do not comment on the Republican decline, though it is incorporated in the long-run equilibrium. Thus comprehensive statistical analyses establish that legislative outcomes in the states reflect both long-term party competition and short-term political events and conditions, but these analyses incorporate the Republican legislative decline statistically without calling attention to it or providing any substantive explanation for it. The analyses reported herein build on these earlier efforts but keep the Republican decline front and center.

As a starting point, let us take a closer look at party control in the state legislatures. As usual, the South is special. The Republicans have *never* won control of a southern or border legislative chamber in the postwar period: all of the action in Figure 2 occurs outside these regions. Thus, I omit the 15 southern and border states from consideration, along with Alaska, Hawaii, Minnesota, and Nebraska.[2] Controlling for short-term variation in the national political climate (measured by interelection change in Gallup approval ratings appropriately signed for the party of the president), the time trend of Republican control in the remaining 31 nonsouthern states is given in Table 1, column 1.

Since World War II, the Republicans have lost legislatures in the non-South at a rate of almost 2% per election. The passage of time is not an explanation, of course, but a context in which true explanatory variables operate. Elsewhere I have suggested that legislative professionalization works in favor of Democrats (Fiorina 1992). Substituting an extremely

TABLE 1

Correlates of Republican Control of State Legislatures: Non-South, 1946–90

INDEPENDENT VARIABLES	MODELS 1	2	3
Constant	60.53	55.93	60.55
	(15.25)	(15.13)	(15.71)
Presidential approval change	−.59	−.58	−.59
	(.21)	(.22)	(.22)
Time trend	−1.74	—	−1.69
	(.57)		(1.61)
Annual sessions	—	−.36	−.01
		(.13)	.36
Republican control (t − 1)	.08	.17	.08
	(.19)	(.18)	(.20)
Corrected R^2	.55	.52	.52

Note: Figures are ordinary least squares estimates N = 22. Time trend: 1946 = 1, 1948 = 2, . . . , 1990 = 23.

crude indicator of professionalization (the simple percentage of states whose legislatures meet annually) for the time trend in Table 1, we get column 2. For every 3% increase in states holding annual legislative sessions, the Republicans lost control of approximately 1% of the state legislatures. The trend in annual sessions tracks the time trend very closely (the simple correlation between the two variables is .95), and when both are included in the equation (column 3), multicollinearity deprives both of significance.

Of course, there are numerous other time series that trend upward (or downward) over this 23-election period. Any of these might perform just as well statistically as the trend in annual legislative sessions. I shall develop the argument that legislative professionalization enhances the electoral prospects of Democratic candidates, then present the evidence.

OPPORTUNITY COSTS AND LEGISLATIVE CAREERS

In the early postwar years, most state legislatures were part-time, minimally supported institutions whose members were poorly compensated. Most were constitutionally limited to sitting every other year. Legislative scholars commonly label such bodies *amateur,* because under these conditions turnover is high, and service is an avocation, not a profession. Some state legislatures still fit this description. The Wyoming legislature, for example, is permitted to meet for 20 days during even years and 40 days during odd years. Members receive $75-per-day salary with a $60 per diem, for a total biennial compensation of less than $7,000. Other perks are minimal. Upper- and lower-chamber turnover rates are similar—about 30% per election in the early 1980s (Niemi and Winsky 1987, tbl. 1).

New York offers a striking contrast. It is a good example of what legislative scholars refer to as a *professional* legislature. In session annually from January through June and again for a period in the Fall, with almost 20 staff per legislator, each member receives a $57,500 annual salary and $75 per diem, as well as travel allowances. Moreover, two-thirds of the assembly and the entire senate receive so-called "lulus," that is, "in lieu of" payments for holding various party and committee leadership posts (Fowler and McClure 1989, 84–89, 96). Assembly turnover in the early 1980s was about 20% per election, with senate turnover under 15%. *Voluntary* turnover was less than 10% (p. 90).

In the early postwar years, most state legislatures fell on the amateur end of the continuum. But over the years, especially during the 1960s, many legislatures have become more professionalized (Kurtz 1991). In particular, legislative service has increasingly become a full-time occupation. Bazar reports that as of 1986, between 11% and 20% of legislators nationwide were full-time and that in some areas (e.g., the middle Atlantic states), over half were professional legislators (1987, 2). My hypothesis is that this development advantages Democratic candidates relative to Republicans. The hypothesis is a logical implication of several propositions previously noted by students of state politics.

First, legislative service does not make equivalent demands on people from different walks of life. Discussing Arkansas (an amateur legislature), Blair comments: "A realtor or banker or lawyer, the owner of a furniture store or car dealership, may have the time flexibility and financial security to adjust his or her schedule to a regular session of two months or more, increasingly frequent special sessions, and countless committee meetings and constituent obligations in between. The realtor's or lawyer's secretary, a bank teller, the furniture and car salespersons who must be at work if they are to receive an income, simply cannot serve" (1988, 164).

Second, if legislative service has an asymmetric impact on people from different occupational and income strata, we should expect *changes* in the conditions of legislative service to change the relative attractiveness of service for different categories of people. That is what Rosenthal reports:

> Minnesota is an example of a state in which marked change in the composition of the legislature has taken place. Lawyers, independent business owners, and farmers have left. Their places have been taken by young people, many of whom see the legislature as an entry level position for a career in politics. But as noticeable as anything else, the contemporary Minnesota legislature has more full-time members. . . . The neighboring state of Wisconsin has moved in the same direction, with younger members and fewer who work part time at the job. There is only a single farmer left in the senate. . . . Legislators themselves are not united on the benefits of professionalization: . . . in California the entire fabric of the process has changed. "The way it used to be, we only had men of means, of experience," a senior senator remarked. "Now we have full-time legislators." (1981, 58)

Finally, we have the common observation that the two parties are based in different socioeconomic strata. Arguing that the Republicans had an advantage in an era of part-time politicians, Key observed:

> Outside the South, the Republican group has . . . a far larger reservoir of persons with leadership skills on which to draw. Well-connected lawyers, businessmen with time and money to devote to politics, and perhaps to a lesser extent, persons with skill in professional politics gravitate in greater degree to the Republican party than to the Democratic. By the secondary network of economic relations within their own group—legal retainers, insurance commissions, real estate transactions, and the like—the business community within the Republican party can sustain a class whose time and energies may be dedicated principally to the practice of politics. . . . The Democratic Party, on the other hand, enjoys the handicaps in recruiting leadership created by its position as a party devoted in principle to mass causes. (1956, 256–57)

Now, as then, the parties have discernibly different socioeconomic bases. As mass surveys monotonously demonstrate, the Republicans have a higher-income, higher-status popular base than the Democrats, especially when the profit sector is distinguished. And elite surveys find that Republican elites come relatively more from the profit sector, Democrats from the public and other nonprofit sectors.[3]

Consider a simplified model based on the preceding propositions. Within a state, two parties compete for public office. Within each party is a pool consisting of all potential candidates, that is, those possessing the minimal qualifications deemed appropriate for service. The parties' candidate pools differ in their occupational and income characteristics. The Republicans are a higher-income, more private-sector party; their potential candidates are independent professionals, proprietors and farmers, and the independently wealthy (as well as their spouses, given that such families are better able to afford domestic substitutes). The Democrats are a lower-income, more public-sector party. Their potential candidates are union officials, teachers, and other public- and nonprofit-sector employees. Given such differences, variations in the degree of legislative professionalism are likely to affect the decisions of those in the respective candidate pools.

Those in the Republican pool are advantaged where the legislature is on the amateur end of the continuum. They are more likely to have the career and financial flexibility to absent themselves from their principal occupations for one to three months a year. Some of their responsibilities can be moved to evenings and weekends or delegated to associates, relatives, or employees. Indeed, for lawyers in private practice, the contacts made in political life raise the value of their outside hours.[4] In contrast, salary and wage earners in the Democratic pool forgo their incomes when they take leave for legislative service—if leave is even feasible. Even if ambition levels are comparable, Republican candidates are more able to pursue their ambitions.

With a professional legislature, the logic reverses. If legislative service is a full-time occupation, legislators must sacrifice outside careers for legislative office. Those who have lucrative private-sector careers will tend to exit legislative service—or never enter—rather than abandon those careers. In contrast, for potential Democratic candidates, legislative service now becomes an attractive alternative career, probably better compensated and more highly regarded than their present careers. Again, even if ambition levels are equal between the respective candidate pools, Democratic candidates are now in a better position to act on their ambitions.

In sum, in amateur legislative settings the critical question is, "Who has the flexibility *to combine* legislative service and an outside career?" In professional legislative settings, the critical question is, "Who is willing *to sacrifice* an outside career for legislative service?" If the career patterns of Democrats and Republicans are as just posited, then, ceteris paribus, the answer to the first question is, "More likely, Republicans," and the answer to the second is, "More likely, Democrats."

Several aspects of the argument deserve additional comment. First, the argument says nothing about net *turnover* in legislatures. There is a substantial literature on that subject, of course. Reformers have supported professionalization partly as a remedy for excessively high turnover, and some research finds that correlates of professionalism (e.g., longer session length and higher compensation) do reduce turnover (Calvert 1979; Rosenthal 1974). The argument made here, however, is that increasing session length and compensation have a differential partisan impact, advantaging Democrats relative to Republicans.

Second, the argument is a *selection* argument. Institutional change (professionalization) has altered the mix of incentives facing legislative candidates so that prospective Republican candidates find legislative service relatively less attractive than in the past, while prospective Democrats find it relatively more attractive.[5] Increasingly, the former select out, and the latter select in. This logic resembles that of Ehrenhalt (1991), though the two arguments have different principal emphases. Noting that Democrats (including those of the most liberal persuasion) sometimes dominate local councils and state legislatures in seemingly conservative locales, Ehrenhalt hypothesizes a difference in party attitudes toward government. Democrats generally believe in government, like to use government to improve society, and consequently enjoy government service; Republicans, in contrast, are skeptical of government capacity, dislike government, and consequently find service much less rewarding. This too is a selection argument. The two arguments are not at all inconsistent. Indeed, they are quite complementary, and Ehrenhalt takes note of the greater career commitment evidenced by Democrats. But I have said nothing about psychological or ideological benefits and costs, only the more tangible costs and benefits of legislative service. Full-time service imposes higher opportunity costs on Republicans, and generous compensation raises the direct benefits of service for Democrats. If the logic of my argument is operative, party fortunes will respond to measures of *tangible* benefits and costs. Ehrenhalt's argument, in contrast, could be valid even in the absence of such relationships.[6]

A third point is that my argument is easy to caricature. I am not claiming that candidates for office look at nothing but the prospective wages and hours, with Republicans being discouraged by longer hours, and Democrats encouraged by higher wages. I am suggesting merely that such considerations affect career choices. On the margins, some qualified Democrats—however public-spirited—will find the sacrifices of service in an amateur legislature too great to bear. Similarly, some qualified Republicans—however public-spirited—will find the sacrifices of service in a professional legislature too great to bear. Such marginal decisions affect the respective candidate pools and, indirectly, the composition of the legislature.

One objection to the foregoing argument is that correlations between occupation, income, and partisanship, while significant, are far from perfect. The objection is well taken, but the empirical question is not the absolute strength of the correlations but whether they are strong enough to contribute to the explanation for the Republican legislative decline. That question can only be answered by looking at the evidence. From past research, we know that candidates' decisions vary systematically with measureable characteristics of the offices they hold, the offices they seek, their attitudes toward risk, and such features of the electoral context as party competitiveness and national forces (Black 1972; Jacobson and Kernell 1981; Rohde 1979; Schlesinger 1966, esp. chaps. 2, 3). This research simply expands the list of factors known to affect candidate decisions.[7]

ANALYSIS

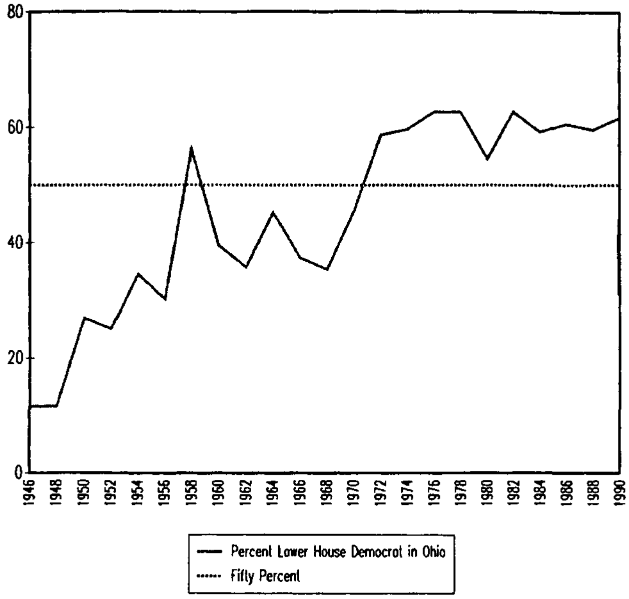
There is a wide-ranging literature on legislative professionalism.[8] Much of it addresses legislative organization and resources (the power of the leadership and the autonomy of committees, the size of legislative staffs, etc.) For purposes of the present research, however, I limit my attention to the two aspects of professionalization that should most immediately affect the individual career decisions of candidates: the length of time that the legislature sits and the compensation for legislative service. Other aspects of professionalism (e.g., size of legislative staffs and autonomy of legislative standing committees) are important for the operation of legislatures and apparently affect incumbent reelection chances (Holbrook and Tidmarch 1991; King 1991; Weber, Tucker, and Brace 1991). My principal attention, however, is on the consequences of candidates opting in and out of the arena, and for that decision, time commitment and compensation would seem to be of greatest relevance. Price (1975) and Ehrenhalt (1991) refer to these aspects of legislative professionalization as simply "professionalism."

The analysis that follows examines party fortunes in the lower houses of 31 nonsouthern state legislatures over the 23 elections between 1946 and 1990. Three variables were selected to represent the career attractiveness of a legislature. First, analogous to the aggregate analysis already reported, I coded a dummy variable (ANNUAL) as 0 for the years in which a state legislature was restricted to biennial sessions and 1 after the change to annual sessions. In the immediate postwar period, only 16% of the legislatures in the analysis had annual sessions. That number more than doubled in the 1950s, more than doubled again by 1972, and stands at 87% today (see Appendix). Despite this sharp trend, I did not expect this gross measure to produce significant relationships in the disaggregated analysis. In the first place, a few states already had annual sessions in 1946—so for them, the variable is a constant for the period of this study. And in the second place, some states that adopted annual sessions experienced only small increases in actual legislative service. For example, in 1974, Wyoming began meeting for 20-day sessions in the even years—hardly the kind of increase that would lead many potential candidates to recalculate the benefits and costs of legislative service. Thus, I expected any effects of annual sessions to be incorporated in a second, more sensitive measure, DAYS.[9] Calculated from successive editions of *Book of the States*, this is simply the total days that the legislature was in session during the election year and the year preceding (see Appendix). While exact session dates are available, this measure nonetheless understates the demands of legislative service, since it does not include committee meetings and constituency service conducted when the legislature is not in session. Given the argument I developed, I expected Republican electoral fortunes to wane as states moved to annual sessions and days in session rose.

Third, again using *The Book of the States*, I calculated the total biennial compensation for legislators, expressed in 1982 dollars. This measure includes legislator base salaries, plus their per diems and other daily allowances multiplied by days in session. Again, the measure is only approximate. It does not capture payments for legislative service (such as committee hearings) while the legislature is not in session. Additionally, "lulus" are not included, although they have come to be increasingly important. Such "in lieu of" payments are hard to factor in since only some legislators receive them. At any rate, since more experienced legislators are usually the beneficiaries, "lulus" probably do not dominate the decisions of new candidates.[10] As explained earlier, we expected Democratic fortunes to wax as real compensation rose. In the early postwar years, average compensation was less than per capita state income, but in the 1950s and 1960s, legislative compensation rose at a much faster rate (see Appendix). State

FIGURE 3

The Case of Ohio



legislators suffered in the inflationary 1970s and prospered in the 1980s.

To illustrate the variation in the three indicators of professionalism, consider the experiences of two states. Figure 3 shows Ohio's interesting postwar history. Prior to 1972, the Republicans won the lower house in every year except 1958. But in the 1972 elections, *with George McGovern heading the ticket,* the Democratic percentage jumped 13 points, and they have been continuously in the majority since then. Did anything obvious happen around the time of this sharp change in partisan fortunes? Yes: at least two developments may be relevant. First, the 1970s reapportionment was carried out by a reapportionment board that had a three-to-two Democratic majority. Perhaps they were extraordinarily clever and produced a massive, lasting Democratic gerrymander.[11] Alternatively, Ohio voters approved a constitutional amendment providing for annual sessions. From a total of 315 days in the 1967 biennial session, the Ohio legislature moved to 717 in the 1971–72 annual sessions.[12] Coincidentally, between the 1968 and 1972 elections, the Democrats scored a 23% seat gain.[13] Simple regressions analogous to those reported earlier in Table 1 indicate that the increased session lengths account for more than half of the Democratic gain.

California (Figure 4) provides a different illustration. Republican hegemony came to an end earlier than in Ohio—in the election of 1958. Since then, Republicans have attained majority status only in 1968. Since California has had annual sessions throughout the postwar period, that variable has no explanatory relevance. While days-in-session has been trending steadily upward, a simple regression analysis analogous to those in Table 1 shows little indication that length of session is related to the improvement in Democratic fortunes. But the third professionalism variable—real compensation—bears a significant relationship to Democratic success. Each $10,000 (biennial) increase in real legislative compensation is associated with a 1% increase in Democratic legislators. Thus, the observed $80,000 increase in real compensation in postwar California directly contributes to a 9% improvement in Democratic fortunes.[14]

While suggestive, individual state analyses are untrustworthy, given the paucity of observations underlying them. By pooling the data, we can carry out a more efficient analysis. The pooled time-series cross section is a rectangular array of 713 observations, somewhat denser in space (31 states) than in time (23 elections). Analysis of such pooled data often runs afoul of ordinary least squares assumptions, namely the homoskedasticity and independence of the residuals. Various correctives have been proposed, but all have their weaknesses. Fortunately, in practice, they seem to produce estimates that are very similar (Stimson 1985). Inclusion of a dummy variable for each state and a lagged variable for the percentage of Democratic representatives in the preceding legislative session should take account of the most obvious sources of difficulty.[15] Based on previous research, we also need to include variables representing national economic conditions, the coattails of candidates for higher offices, and the on-year-off-year cycle.

Table 2 reports the estimates for several related specifications of a dummy variable least squares model. In these specifications, each state has its own intercept (the constant is omitted). In effect, each state is allowed to have a different average *level* of

FIGURE 4

The Case of California

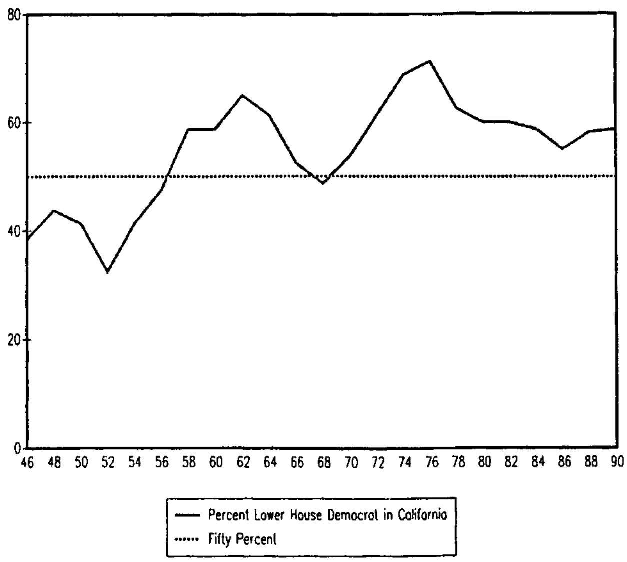


TABLE 2

Democratic Seat Share in Non-southern Lower Houses, 1946–90

INDEPENDENT VARIABLES	MODELS 1	2	3	4
Year	.004 (.047)	—	—	—
Democratic Reps. (t − 1)	.505 (.029)	.505 (.028)	.512 (.027)	.515 (.027)
Annual	1.05 (1.19)	1.11 (1.04)	—	—
Days-in-session	.002 .004	.002 (.0038)	—	—
Compensation				
Real	.0071 (.0042)	.0073 (.0039)	.0099 .0030	—
Relative	—	—	—	1.21 (.363)
Presidential				
Year	−27.70 (2.81)	−27.68 (2.80)	−27.29 (2.78)	−27.14 (2.78)
Vote	.5199 (.0610)	.5191 (.0604)	.5086 (.0598)	.5026 (.0597)
Gubernatorial				
Year	−20.13 (2.78)	−20.18 (2.74)	−20.14 (2.74)	−20.08 (2.74)
Vote	.3986 (.0529)	.3993 (.0523)	.3981 (.0522)	.3968 (.0522)
Off-year	−9.76 (1.06)	−9.78 (1.03)	−9.98 (1.02)	−10.11 (1.01)
GNP growth	.1731 (.0628)	.1732 (.0628)	.1670 (.0625)	.1501 (.0622)
SEE	9.02	9.01	9.01	9.01

Note: Standard errors are in parentheses. N = 712. Corrected R^2 = .735.

Democratic representation, although a common *dynamic* is presumed. (However, the 31 state intercepts are not reported.) Column 1 includes all the variables discussed so far.[16] The time trend (YEAR) is effectively zero, suggesting that the substantive variables in the analysis capture all the temporal variation in the data.[17] Democratic strength in the preceding session (DEMOCRATS (*t* − *1*)) is of major importance. The coefficient of .5 might suggest less dependence on past history than one might have anticipated, but coefficients of such lagged terms are generally biased downward in this sort of analysis. National economic conditions, measured here as the rate of growth of gross national product in the election year are significant, consistent with the findings of Chubb (1988).[18] On average, every 1% gain in gross national product during a Democratic (Republican) administration adds (subtracts) about a sixth of a percentage point to (from) Democratic state legislative strength. The coattail specifications follow Chubb: presidential and gubernatorial coattails are each represented by (1) a dummy variable equal to 1 for the election year and (2) a variable for the actual vote in the state. This specification differentiates between years in which there is no election (vote = 0) from years in which the Democratic presidential or gubernatorial candidates perform poorly. Thus, presidential coattails are estimated as 0 in nonelection years, and −27.70 + .520 (PRESIDENTIAL VOTE) in election years. Solving, Democratic state legislators are collectively better off running in a presidential election year than in an offyear, so long as their presidential candidate wins more than 53.3% of the statewide vote. For gubernatorial candidates, the figure is 50.5% of the vote. Given the performance of Democratic candidates other than Lyndon Johnson in the postwar period, it appears that they have not been of much help to their state legislative compatriots. Republican presidents, however, have been quite helpful: the coefficient of OFFYEAR (−9.75) indicates that on average, Democrats win almost 10% percent more state legislative seats in midterms when a Republican is president than when Democrats hold the White House. This result is consistent with either a "presidential penalty" model (Alesina and Rosenthal 1989; Erikson 1988) or a "surge-and-decline" model (Campbell n.d. [6]).

Of most interest are the three indicators of professionalism. Annual sessions and days-in-session have the expected sign, but their coefficients are smaller than their standard errors. Real compensation, however, is just significant at the .05 level (one-tailed test). The coefficient indicates that each $10,000 increase in real biennial compensation results in .7% more Democrats.

Column 2 omits the insignificant time trend. The coefficients change little but their standard errors become a bit more precise as multicollinearity lessens. Annual sessions and days in session continue to fall far short of significance, but the case for real compensation becomes slightly stronger. Column 3 omits "DAYS" and "ANNUAL."[19] Absent their interference, the coefficient of real compensation rises to almost 1% more Democrats for every $10,000 and the estimate is more than three times its standard error. Finally, if potential candidates define their career alternatives within state boundaries, the more appropriate measure might be legislative compensation relative to some state norm. Column 4 reports estimates of an equation that includes real legislative compensation relative to real per capita state income. The coefficient is again three times as large as its standard error. Thus, whether we measure it in absolute or relative terms, legislative compensation clearly matters. To illustrate, real biennial compensation in California has increased from approximately $10,000 to more than $90,000 in the postwar period. Applying the national coefficients from column 3, this gain in compensation should have led directly to a Democratic legislative gain of 8 + %. Alternatively, real legislative compensation relative to real per capita disposable income in California was five times higher in 1990 than in the early postwar years. According to column 4, the predicted Democratic gain is about 11%.

OBJECTIONS

Increasing legislative compensation and increasing Democratic representation are apparently related. Could the relationship be spurious? The most obvious possibility is that both variables could be correlated with some omitted third variable. For example, some commentators have hypothesized a pro-Democratic trend in political sentiments in nonsouthern state elections. It is difficult to test such a possibility directly: registration or other measures of partisanship are incomplete both by state and by year. At any rate, once substantive variables are taken into account, there is no trend in Democratic state legislative fortunes (column 1). Thus, proponents of a "trend" hypothesis would have to argue that some unobserved drift in partisan sentiments happens to bear a close relationship with the observed trend in real legislative compensation. The trend in compensation is a more parsimonious explanation for the trend in Democratic fortunes than is some hypothetical unobserved trend in partisan sentiments.

Alternatively, one might object that legislative compensation is just a reflection of state population, state income, and size of state government. Bigger, richer, more active states pay their legislators more, and such states—the populous, urban, industrial states—tend to be more Democratic. These two propositions are certainly true, but that does not make the relationships reported in Table 2 spurious. In the first place, these propositions were as true in 1952 when the Republicans won 55% of the presidential vote and 26 of 31 nonsouthern legislatures as they were in 1988 when the Republicans won 54% of the presidential vote but only 9 of 31 nonsouthern legislatures. If there were no temporal correlation between rising compensation and *increasing* Democratic strength, we would only find larger intercepts for the urban, industrial states. In the second place, if we add state sociodemographic variables to the analysis, we find that they do not eliminate or even weaken the estimated effect of legislative compensation: in fact, the coefficient of legislative compensation marginally increases when state population, personal income, and state government expenditures are added to the model ($b = 1.06$, $t = 2.38$). Moreover, the coefficients of the additional control variables are smaller than their standard errors, and expenditure has the "wrong" sign. Thus, the state intercepts and the variables previously included in the analysis fully take into account interstate differences in socioeconomic development.

Finally, what of the fact that the upsurge in Democratic legislators ceases in the early 1970s? Part of the answer may be that real legislative compensation also stopped rising after 1972 (see Appendix), although it resumed its increase after 1982. Another possibility is that the effect of compensation is nonlinear, being stronger up to the level where legislative service becomes competitive with alternative Democratic career opportunities, but weakening thereafter. Taking the log of real compensation, however, resulted in a poorer specification than the linear one. And several interactive specifications yielded no significant results. Why the Democratic gains ceased after the early 1970s must remain a question for future research.

TABLE 3

Democratic Vote Share in Nonsouthern Gubernatorial Races, 1946–90

INDEPENDENT VARIABLES	MODELS 1	2	3
Year	.023 (.032)	—	—
Democratic vote ($t - 1$)	.324 (.047)	.334 (.045)	.340 (.045)
Compensation			
Real	.0022 (.0027)	.0026 (.0026)	—
Relative	—	—	.1028 (.1717)
Presidential			
Year	−8.43 (3.15)	−8.46 (.3.15)	−8.49 (3.15)
Vote	.144 (.067)	.138 (.067)	.135 (.067)
Off-Year	−4.83 (1.02)	−5.13 (.929)	−5.29 (.942)
GNP Growth	.0786 (.0635)	.0078 (.0635)	.0742 (.0648)
Corrected R^2	.228	.229	.228

Note: Standard errors are in parentheses; n = 465; SEE = 7.63.

CONTRAST WITH GUBERNATORIAL ELECTIONS

Why have Republican fortunes plunged in legislative elections but fluctuated without trend in executive elections? The office of governor is and always has been full-time, prestigious, and respectably compensated.[20] Thus, unless party competition is so unbalanced (e.g. as in the South) that every aspiring politician must seek a career within the dominant party, we would expect that the gubernatorial candidate pools are and have been more evenly balanced than the legislative pools. In particular, compensation levels were not so low early on as to disadvantage Democrats nor later on so low relative to the private sector as to discourage Republicans. Thus, real compensation should *not* have any impact on partisan fortunes in gubernatorial elections.

Gubernatorial elections have been much studied and are known to reflect state party strength, the national economy, and national party fortunes (Campbell 1986; Chubb 1988; Simon 1989; Simon, Ostrom, and Marra 1991). To variables of this sort I add real gubernatorial compensation (Table 3). The resulting estimates are consistent with earlier, more elaborate studies in finding that presidential coattails, the off-year penalty, and previous support levels are important.[21] As anticipated, however, the effect of real compensation, whether measured absolutely or relative to state income, has only a weak and insignificant effect.

IMPLICATIONS FOR CONGRESS

I do not think that a simple extension of the present argument can help to explain the persistence of Democratic majorities in the modern U.S. House. The New Deal realignment and resulting distribution of party identification would seem to be a sufficient explanation for Democratic House majorities between 1952 and the mid-1960s. The more interesting question is why Republican House candidates have not fared better during the more recent elections when their presidential candidates were faring so well. Possibly, falling congressional salaries (in real terms) and rising executive and professional compensation discouraged some potential Republican candidacies, but given the prestige of the office and the absolute level of compensation, the argument seems somewhat strained.[22]

Still, professionalism may have contributed *indirectly* to the stability of the Democratic House majorities. State legislatures have long been considered "farm teams" for Congress. State legislatures provide the opportunity for inexperienced politicians to learn how to nurture a constituency and solidify an electoral base. The more talented and ambitious of these eventually have opportunities to move up to the national legislature. Despite suggestions that service in some state legislatures may now be preferable to service in Congress (Fowler and McClure 1989), state legislatures remain the principal springboard to Congress. Indeed, Canon (1990) reports that state legislatures have become an even more important source of congressional recruits in recent years. Thus, the decline in Republican fortunes at the state level has probably contributed to their chronic difficulties at the congressional level. Jacobson attributes the lack of Republican congressional success in the 1980s to their failure to run "good" candidates, where "good" is defined as previous electoral success (1990, chap. 4). By the 1970s, the Republicans had fewer state legislators in general and fewer in positions of legislative leadership in particular. Their lack of good candidates in the 1980s may well have been a by-product of their declining fortunes in state legislatures during the preceding two decades.

CONCLUSION

While the work reported herein is preliminary, the relationships uncovered seem robust enough to merit further investigation and the argument plausible enough to merit further development. Several lines of research are prime candidates for further work.

First, the empirical research reported here is an indirect test of the general argument. The outcomes of state elections are consistent with the implications of the argument, but focused research in the states could shed additional light on the hypothesized mechanisms. If professionalism makes legislative seats more attractive to Democrats and less attractive to Republicans, then, ceteris paribus, as professionalism increases, does the number of contested Democratic primaries increase, and Republican, decrease, and does the number of uncontested Democratic races increase, and Republican, decrease? Do retirement rates of Republican incumbents increase relative to Democrats as professionalism increases? Do Democratic candidates look more impressive and Republican candidates less impressive as professionalism increases? Such patterns would constitute more direct evidence that candidate career calculations are indeed the mechanism underlying the patterns reported herein.[23]

Second, was there any partisan political impetus for professionalization of state legislatures? I have taken professionalization as exogeneous in this study—a result of socioeconomic development and a nonpartisan reform impulse. But partisan politicians can be expected to see the partisan implications of nonpartisan reforms: Did they, in the case of professionalism? Were Democrats more likely to support and Republicans to oppose the move toward full-time, reasonably compensated legislative service? Analysis of the data reported herein finds that both session lengths (significantly) and real compensation (not significantly) are positively related to the proportion of Democrats in the previous legislature (controlling for the previous values of session lengths and compensation as well as the socioeconomic characteristics of the state). If further research supports this finding, we might conclude that Democrats tend to encourage professionalism, but whether either party consciously anticipated a partisan impact is a different question. Democrats may well have done what they could to make public office a full-time position without suspecting that it would give them an advantage relative to Republicans. Again, however, this is an indirect way to look at the question. There are records of legislative deliberations and constitutional amendment campaigns: Who supported and who opposed what? Were partisan divisions present in roll calls and referenda? Or were the decisions to extend sessions and raise compensation more often regarded as nonpolitical, procedural matters?

Third, I reiterate the neutrality of my findings. Higher levels of legislative compensation appear to have made service in state legislatures relatively more attractive to Democrats and relatively less attractive to Republicans. This dynamic has contributed significantly to the great turnaround in party fortunes in state legislative elections since World War II. Such a general finding would appear to be fraught with political and partisan implications. In particular, frustrated Republicans may jump to the conclusion that state legislatures have been captured by greedy Democrats who consider the legislature a good job. No such conclusion follows. Judgments of over- or underrepresentation presume some objective baseline. Perhaps Democrats are overrepresented now, while Republicans were overrepresented earlier. But Democrats might be properly represented now because Republicans were so greatly overrepresented earlier. We cannot tell without some independent measure of the "proper" level of representation. It is tempting to use the vote for some statewide office like auditor or attorney general as a measure of partisan sentiment, but districting arrangements would still muddy the representational picture. The residential concentration of one party's supporters may make it inevitable that it "wastes" more of its votes and wins fewer legislative seats than its level of popular support indicates. Judgments about appropriate levels of representation are exceedingly complex and require more information than anything reported here.

Fourth, to the extent that terms-limitation initiatives also significantly limit days-in-session, legislative compensation, and legislator perks, such initiatives should make the legislatures more amateur and, quite likely, more Republican. But if service limits are imposed on legislatures without changing much else, little partisan impact should be expected: in amateur legislatures term limits would bind few members (Moncrief et al. 1992), and in professionalized legislatures, 8 to 12 years in office should be a sufficiently long lead time that even the most risk-averse Democrat will not be discouraged.

Finally, one of the perennial themes in the literature of political science is that political reforms have unintended consequences. In the professionalization of state legislatures we appear to have another example of a seemingly nonpartisan, "nonpolitical" reform, some of whose consequences may well be highly partisan and highly political.

APPENDIX

Figure A-1 shows the percentage of non-southern states holding annual legislative sessions doubling between World War II and the mid-1960s and doubling again by 1980.

Figure A-2 shows session lengths in non-southern legislatures increasing steadily between World War II and 1980. Currently such legislatures sit for an average of 350 days during each two-year inter-election period.

Figure A-3 shows that legislative compensation was lower than per capita income in the early postwar period, rose steadily in the 1950s and 1960s, plateaued in the 1970s, and then resumed its increase in the 1980s.

FIGURE A-1

Percentage of 31 Nonsouthern States with Annual Sessions

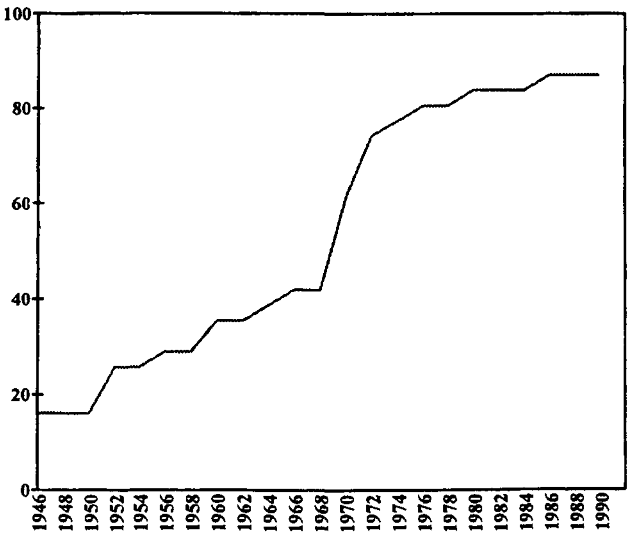


FIGURE A-2

Average Number of Days-in-Session in 31 Nonsouthern Legislatures

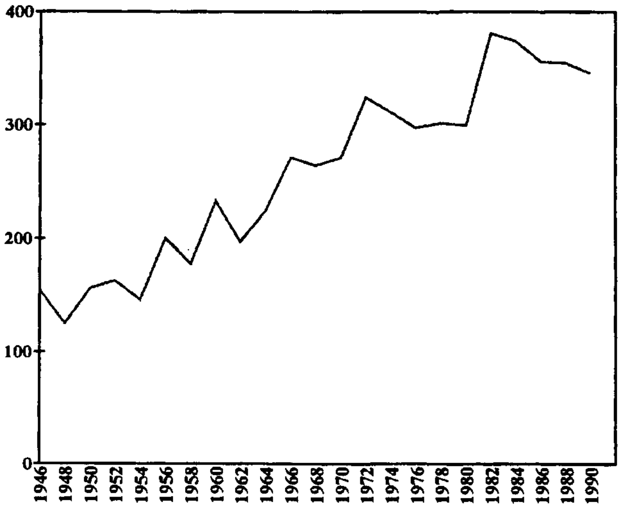
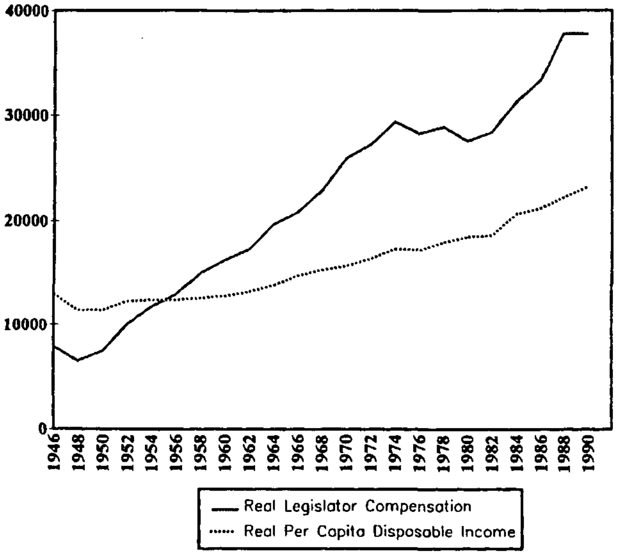


FIGURE A-3

Average Biennial Compensation Versus Per Capita Disposable Income in 31 Nonsouthern States (1980 Dollars)



Notes

This research has been supported by grants from the National Science Foundation (SES-91-12648) and the Joseph B. Grossman Fund. I wish to thank James Alt, Neal Beck, Derek Bok, Alan Ehrenhalt, Jay Greene, Ronald Hedlund, Gary King, Keith Krehbiel, Karl Kurtz, Roger Noll, Theda Skocpol, James Q. Wilson, and Mark Zupan for helpful comments.

1. Karl Kurtz (personal communication) has suggested that I differentiate *professionalization* from a *Professionalization* reform movement. The former refers to ongoing developments (however caused) that make state legislatures more like the Congress, whereas the latter refers specifically to good-government reform efforts, especially those of the 1960s.

2. Nebraska has nonpartisan legislative elections, Minnesota was nonpartisan before 1972, and Alaska and Hawaii had their first elections as states (1958 and 1959, respectively) after a significant part of the action in Figure 1 had already occurred.

3. On the well-known socioeconomic differences in the mass bases of the parties, see Miller and Traugott (1989, tbls. 2.4, 2.8, 2.18). On differences in party elites (convention delegates) see, e.g., Stanley and Niemi 1990, tbl. 4-10). More recently, Black and Black report that 45% of the delegates to the 1992 Democratic convention were employed by government in some capacity (1992, 5). (Unfortunately, they do not report a figure for Republicans, but it is undoubtedly lower.) I have been somewhat surprised by the scarcity of comprehensive data on occupational characteristics of party elites. In one case, for example, excellent data exist but are not reported separately by party (Bazar 1987).

4. Keefe and Ogul observe that for lawyers, "political involvement may well bring an unearned increment by attracting, through publicity and social visibility, new clients and higher fees" (1993, 140). Interestingly, Bazar reports a sharp drop in the number of lawyer legislators between 1976 and 1986, citing "the time requirements of legislative office," stringent disclosure laws, and the legalization of advertising as contributing factors (1987, 3).

5. There is nothing novel about this argument. Classic references include Aristotle, who took note of the claim that the Athenian law courts deteriorated after pay for service attracted a more "common" kind of men (Fritz 1964, 97–98), and J. S. Mill, who opposed parliamentary compensation on grounds that it would discourage successful professionals and attract "adventurers of a low class" (Acton 1972, 311). Indeed, the standard argument in favor of decent legislative compensation is that in its absence, legislative service is the privilege of the rich, who presumably are not representative of the entire polity.

6. There is an obvious synthesis of the two arguments: desiring full-time careers in government, Democrats work to professionalize state and local offices. Although that argument is beyond my scope here, I shall offer some brief remarks.

7. Francis (1993) has shown that representatives in high-paying state legislatures are significantly more likely than those in low-paying legislatures to wait for open seats rather than challenge incumbent senators. This finding suggests too that career decisions do depend on mundane wages and hours considerations.

8. The Citizens Council on State Legislatures has published numerous reports advocating the professionalization of state legislatures. The seminal articles relating professionalization to public policy are by Grumm (1970, 1971). Hedlund (1984) provides an exhaustive review of studies that relate professionalization to legislative organization and policymaking.

9. Obviously, legislatures that meet annually spend more days in session, but the correlation is far from perfect.

10. A third possible omission is allowances for travel, postage, and so forth which are usually expressed as formulas rather than payments actually made. If such allowances are reasonable and members are honest in claiming them, no understatement of compensation will arise from this source. If not, an additional income source will be overlooked.

11. The aggregate statewide vote for lower House Democrats jumped from 46% in 1968 to 54% in 1970 and fell slightly to 53% in 1972. The 1972 gain in seats with no gain in votes over 1970 is consistent with the gerrymandering hypothesis. The 7% jump in votes between 1968 and 1972 (even while McGovern was drawing little more than a third of the statewide vote) is consistent with an improvement (decline) in the quality of Democratic (Republican) candidates.

12. As far as I can determine, the legislature had been "fudging" somewhat by recessing at the end of the odd-year session and meeting for a time in the even years. In the intervening 1969–70 legislature, they sat for 414 days by extending the session into the even year.

13. Interestingly, a contemporary observer (Asher 1978) speculated that these developments would have unintended consequences. Asher cautioned that professionalization in Ohio would lead to insulation of incumbents, as was apparently occurring in the U.S. House of Representatives.

14. Lest some prejudiced readers dismiss this finding as a reflection of the stereotypical materialism of Californians, I point out that the analogous regression estimated for Wisconsin, purported to be one of our more altruistic states, yields a coefficient (.022) precisely *twice* as strong as California's.

15. According to Stimson, "Where unit effects are present and concern for estimator efficiency is not paramount (e.g. when estimating from very large samples) Least Squares Dummy Variables is a reasonable choice" (1985, 945). Neal Beck has examined the variables used in this data set for classroom purposes and reports that they are suitable for "garden variety analysis" (personal communication).

16. There are five intercorrelated right hand side variables: *time, lagged Democrats, days-in-session, annual sessions,* and real income.

17. In addition to a linear time trend, I also checked for a pro-Democratic shift coincident with the "reapportionment revolution." No such shift was found. Previous studies reported only small Democratic seat gains in the wake of *Baker* v. *Carr* (e.g., Erikson 1971). My analysis suggests that even such small gains may have been attributable not to reapportionment but to the rapid professionalization that took place in the late 1960s (see Appendix). That development was not apparent to scholars at the time.

18. Simon, Ostrom and Marra, (1991) found no direct effect of economic variables with presidential approval included in their equations. Before 1950, Gallup did not ask the approval item during the campaign, but if we reestimate the equation for column 1 for 1950 onwards and include Gallup approval, gross national product growth is not significant—consistent with the findings of Simon, Ostrom, and Marra.

19. Neither DAYS nor ANNUAL approaches significance, with real compensation in the equation. With real income omitted their coefficients are greater than twice their standard errors. Numerous estimations leave little doubt that compensation is the most important of the three (though compensation, of course, is partly a function of days in session).

20. Comparing the three postwar elections with the three most recent elections, average real legislative compensation has increased 500%, whereas average real gubernatorial compensation has increased only 25%.

21. National economic conditions are not, however, in contrast to Chubb's findings. Apparently, the OFFYEAR variable included in this analysis captures the decline in economic prosperity that often occurs in midterm years.

22. By the mid-1970s, congressional salaries fell to a level of six or seven times per capita income, lower than at any time since 1920 (*Congressional Quarterly Weekly Report* 1989). In real terms, congressional salaries declined between 1972 and 1984.

23. A confounding factor in such tests is the increasing importance of incumbency in state legislative elections (Jewell and Breaux 1988). For example, rising compensation might generate more "potential" Democratic candidates, but if incumbency effects grow stronger at the same time, the number of primary challengers for Democratic incumbents and general election challengers of Republican incumbents might actually decrease overall. Research clearly would have to control for incumbency as well as other obvious factors like majority status.

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American Political Science Review Vol. 88, No. 2 June 1994

RACIAL THREAT AND PARTISAN IDENTIFICATION

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Over the past three decades, as the Democratic party in the South has come to depend more heavily on black voters for its success, it has experienced a decline among white adherents. Power theory *views relationships between groups as a function of their competitive positions in political, economic, and social arenas. In contexts where the threat posed by a minority group is high, the dominant group's response is predicted to be more hostile than in contexts where that threat is low. A pooled time series analysis of voter registration data for Louisiana parishes for 1975–90 provides support for the operation of the threat mechanism. Higher black concentrations are associated with declines in the percentage of white registered voters who are Democrats and an increase in the percentage who are Republicans. Consistent with the expectations of power theory, this relationship is conditioned by the social status of the parish.*

The past three decades have witnessed two dramatic trends in southern politics. First, beginning with the Voting Rights Act of 1965, there has been an extraordinary increase in political participation among southern blacks. This mobilization has been directed almost entirely to the Democratic party. Second, commencing at about the same time, a steep decline has occurred in the identification of southern whites with the Democratic party. This decline has been accompanied by a limited growth in Republican identification in the region and a large increase in the number of southern whites who identify as Independents. The end product of these trends has been twofold. On the one hand, the once solidly Democratic South no longer can be said to have a majority party (Black and Black 1987, 232). On the other hand, the Democratic party that once championed racial segregation in the region has come increasingly to depend upon black voters for its success and, thus, to reflect a more multiracial set of policy preferences.

Several authors have suggested that there is a relationship between the rise of black influence within the Democratic party and the decline of that party's fortunes among white voters (Black and Black 1987; Carmines and Stimson 1989; Huckfeldt and Kohfeld 1989; Lamis 1988, 299; Parent 1988, 209; Sundquist 1983, 400–403; for an opposing view, see Beck 1977; Stanley 1987; Stanley and Castle 1988).[1] One source of theoretical support for such a linkage can be found in the *power theory* of intergroup relations (Giles and Evans 1986).[2] This approach views relationships between groups as a function of their competitive positions in political, economic, and social arenas. This competition is conceptualized as contextually conditioned. In contexts where the threat posed by a minority group is high, the dominant group's response is predicted to be more hostile than in contexts where that threat is low. The operation of such a dynamic in American race relations has been supported by research linking the level of black concentration in an area to the level of various forms of white hostility; lynching (Corzine, Creech, and Corzine 1983), resistance to school desegregation (Giles, Cataldo, and Gatlin 1975; Pettigrew 1957), attitudinal support for racial integration (Fossett and Kiecolt 1989), and voting for racist candidates (Black 1973; Wright 1977).

Of the advocates for a linkage between black influence and white decline in the Democratic party, the argument of Huckfeldt and Kohfeld (1989) most closely parallels that of the power approach. They argue that the defection of white Democrats is a direct response to the threat that black voters pose to white hegemony within the party, and they conceptualize this *black threat* as contextually specific (pp. 46–47, 106). And, "the severity of racial incompatibility within the [Democratic] coalition, and the attendant unraveling of the coalition, are dependent upon the relative size and intersection of racial groups in particular settings" (p. 41). Moreover, they view the process of threat and defection as self-accelerating. As whites defect from the Democratic party, the party becomes more heavily dependent upon black support which in turn leads more whites to defect: "Racially polarized politics is not a simple function of individually based racist impulses. It is also the product of the progressive transformation of party coalitions that is consequent of these antagonisms" (p. 106). Huckfeldt and Kohfeld find support for their thesis in the 1984 presidential election where the percentage of whites voting for the Democratic candidate decreased as the percentage of blacks in the state's coalition increased (1989, 52).

The power approach leads us to expect that members of the dominant group situated in higher-threat contexts will respond more negatively toward the minority than will those situated in lower-threat contexts. However, since the extent to which members of the dominant group compete with the minority varies, the effect of minority threat may not be constant across all members of the dominant group even under similar contextual circumstances (Giles and Evans 1986). Those dominant group members most directly in competition with the minority are expected to be most sensitive to the level of threat that it presents to dominant group power in a particular context. Since blacks are more likely to be employed in lower-status unskilled and semiskilled occupations, whites in similar occupations are more directly threatened by black advances. Moreover, lower-status whites also are more likely to compete with blacks for housing and for control of public facilities, such as schools and parks (Giles and Evans 1985; Labovitz and Hagedorn 1975). Thus, the power approach suggests that white reactions to the growth in influence of blacks within the Democratic coalition is conditioned by social status. Lower-status whites are expected to be more sensitive than higher-status whites to this threat and thus more likely to leave the party. Consistent with this expectation, Huckfeldt and Kohfeld found that the negative relationship between the percentage of whites voting for the Democratic candidate in 1984 and the percentage of a state's Democratic coalition that was black was stronger among low-income whites than among high-income whites (1989, 49).

The relationship between social class and Democratic support, however, may not be as straightforward as suggested by power theory. Black threat constitutes a force pushing lower-class whites toward exiting the Democratic party but there has been little to pull this group toward the Republicans (Huckfeldt and Kohfeld 1989). With the exception of their appeals to fundamentalists, the Republicans have not actively recruited support from among white lower classes. They have instead built their electoral coalition from the apex of the the social structure downward (Black and Black 1987, 249, 275; Sundquist 1983, 364–67). This may be particularly true for Louisiana in the 1980s, where Parent sees the Republican party as a coalition of white-collar, affluent suburban whites (1988, 216). Thus even if lower-status whites are more sensitive to black threat, this may be counteracted by a perception of the Republican party as not responsive to their interests. The final result could be that lower-status whites remain in the Democratic party despite their concerns or that they exit to become Independents rather than Republicans.

Racial sensitivity within a dominant group also may not be constant across time. Events occurring either within or outside of the local context may cause the salience of race—and hence the response of whites to racial threat—to vary. One such event in recent political history was the presidential candidacy of Jesse Jackson. Huckfeldt and Kohfeld argue that the candidacy of Jesse Jackson "could not help but increase the salience of race as a dividing line between Democrats and Republicans" (1989, 108). This theme was reiterated by Tyrone Brooks, a black Georgia state legislator, "I believe Jackson's candidacy may in fact contribute to the further alienation of conservative whites and Democrats who will go and vote Republican in the General Election" (quoted in Lamis 1988, 299). Thus, we would expect a surge of white defections from the Democratic party during the 1984 and 1988 presidential campaigns.

The present study reexamines the relationship between black political threat and the decline in white support for the Democratic party. Adopting the power approach, we hypothesize that as blacks constitute a larger component of a given political context, defection of white voters from the Democratic party will increase. Moreover, we hypothesize that this relationship will be conditioned by white social class and will vary temporally with the salience of race in the political arena.

MODELING DEFECTION

Official voter registration data for all 64 Louisiana parishes (counties) in the time period from 1975 through 1990 are employed to examine these hypotheses. Given its contextual nature, testing the power hypothesis necessitates information on the partisan identification of whites within areal units. Voter registration data in Louisiana are particularly well suited for this purpose. Since 1975, the state of Louisiana has provided data on the number of registered voters in each parish broken down by race and party.[3] Louisiana is one of only two states to provide such information and the only state for which such data are available on an annual basis.[4]

Louisiana is also an interesting case because of its adoption of an "open" primary system in 1975 (Lamis 1988, 112–13; Parent 1988, 212–13). Under this system candidates from all parties run in the same primary. The candidate who receives a majority of the votes cast is elected to office. If no candidate receives a majority, then a general election is held between the top two candidates. Unlike a "closed" primary, the Louisiana system does not penalize registration as an Independent nor registration as a member of a perpetual minority party. For example, a voter so disposed can register as a Republican and still participate in the choice between two Democratic candidates for the governorship.

The adoption of the open primary in 1975 makes that year the reasonable starting point for examining the effect of black threat on growth in Republican registration. Black registration increased dramatically in Louisiana after the 1965 Voting Rights Act, growing from approximately 240,000 in 1966 to approximately 400,000 in 1975. This growth constituted an increased threat to white hegemony within the Democratic party but, given the monopoly that the Democratic party maintained over office holding, the closed primary meant that registering as an Independent or a Republican essentially removed the voter from effective participation in electoral politics. As a result, Republican registration increased prior to 1975, but the growth in absolute numbers was fairly small from approximately 20,000 in 1966 to slightly more than 50,000 in 1975. The principle response of whites to increasing black threat during this period was not to desert the Democratic party but to countermobilize. Thus, while the percentage growth in the registration of black voters exceeded that of whites between 1966 and 1975 (42% and 25%, respectively), the growth of white registrants in absolute terms was twice that of black registrants (361,000 and 177,000). It is noteworthy that the tendency of whites to mobilize in response to black threat continued in the period under study. Between 1975 and 1990, parishes with higher black populations were characterized by a higher percentage of registration among the white voting-age population. The higher the black concentration in the population, the more likely eligible whites were to register to vote.[5]

Most studies focusing on changes in partisan identification have relied either on survey responses or electoral results. Both of these are susceptible to the short-term factors that surround elections and so may indicate more volatility among the electorate than actually exists (Brodsky 1988). Voter registration, which requires an expenditure of time and effort by the individual, would appear to be less susceptible to such short-term factors. On the other hand, the requirement of effort, as well as strategic considerations, suggests that some voters will maintain official registration with one party while voting for, and attitudinally identifying with, another. Absent major strategic considerations, like the closed primary, the pressure for cognitive consistency should motivate such individuals eventually to alter their registration to match their voting behavior and attitudes. This movement toward consistency, however, need not be rapid or complete. Thus, official voter registration statistics probably present a conservative estimate of voter change and thus a conservative test of the hypotheses. This conservative bias is to some extent offset by the richness of the voter registration data for Louisiana, which provide more detailed longitudinal information than is available through the use of either surveys or election results.

White defection from the Democratic party can take two forms: movement to Independent identification and movement to Republican identification. These are measured straightforwardly as the percentage of white voters registered as Independent and Republican in a parish for a given year. Since both the relationship between these two forms of defection and the relation of each to black threat is uncertain, they will be treated as separate dependent variables in the analysis.

One possible measure of racial threat to white political hegemony is the percentage of the total Democratic registration that is black. This provides a precise measure of the dependence of the Democratic party on black voters and thus of the racial threat to white control of the party. There are two potential problems with employing this operationalization in the present study. The first is that an arithmetic relationship exists between racial threat operationalized in this manner and the dependent variable of interest.[6] Whites may be leaving the Democratic party for a variety of nonracial reasons (e.g., more conservative positions regarding defense policy), rather than because of higher black concentrations in the Democratic party, and these departures, by reducing the Democratic base (i.e., the denominator) necessarily results in a higher percentage of blacks within the Democratic party. Simply put, the causal flow may be from growth in Independent and Republican identification to increases in the percentage of blacks within the Democratic party, rather than vice versa, as hypothesized here.

The second problem with using this operationalization is that it requires a presumption that whites are aware of and sensitive to the racial balance among registered voters. While the relevant context for assessing a contextual hypothesis is often problematic, it would seem more likely that whites would be aware of and sensitive to the racial balance in the population at large than among voter registrants.

Given these concerns, racial threat is operationalized in the present study as the percentage of the voting-age population in each parish that is black. Conceptually, this is an indicator of the potential threat that blacks pose to white political hegemony in each parish generally and within the Democratic party specifically. Empirically, this measure of potential threat is strongly linked ($r = .82$) to the actual percentage of registered Democrats in each parish who are black. The lack of a perfect relationship between these two measures probably reflects the operation of the arithmetic process noted, as well as social and political processes relevant to mobilization (e.g., parish differences in socioeconomics, the presence of voter registration organizations).[7]

Since population data are only available through the census, interpolation was employed to estimate the size of the total and black voting eligible populations in the years between 1970, 1980 and 1990. In the analysis that follows, racial threat is modeled as having a one-year lagged effect. This allows for a reasonable delay between recognition of the level of racial threat and the response of registering as an Independent or Republican.

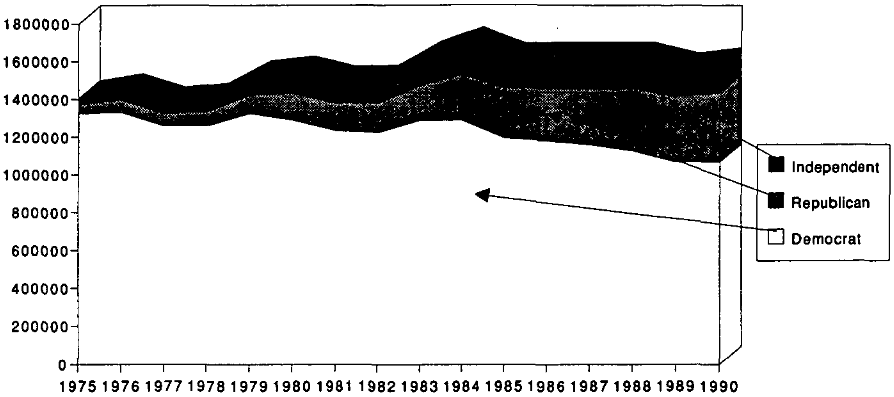
As noted, considerable research has found racial concentration to be perceived as a threat by southern whites. Much of this research, however, focused on earlier time periods. It might be argued that after prolonged experience with blacks as voters, whites no longer perceive higher concentrations of potential black voters as a threat. The evidence for Louisiana indicates that this is not the case. White electoral support for David Duke, an avowed racist, in his candidacy for the U.S. Senate in 1990 and the governorship of Louisiana in 1991 was positively linked to the percentage of registered voters who were black (Giles and Buckner 1993). The greater the concentration of blacks among the registered voters in a parish, the greater the percentage of registered whites who voted for Duke. Thus, the evidence supports the operationalization of racial threat in terms of the level of black concentration.

The influence of the Jackson candidacy is captured by two dummy variables. The first is scored 1 for 1983 and 1984 and 0 for the remaining years, and the second is scored 1 for 1987 and 1988 and 0 for the remaining years.

The social status of whites within the parishes is

FIGURE 1

White Voter Registration by Party in Louisiana, 1975–90



measured using white median household income as reported in the 1980 census. The decision was made to use "household" rather than "family" income since the former more closely taps the social structure present in the living quarters of individuals. Since the principal focus is on lower-class whites, information was also collected on the percentage of whites below the poverty level in the parishes. As might be expected, this indicator is strongly related to median income ($r = -.85$). Accordingly, only median income is employed in the following analysis.

Quite apart from the racial threat in a particular parish, growth in Independent and Republican identification may reflect statewide and national trends. This factor is taken into account by the inclusion of a simple "counter" variable set to 0 in 1975 and incremented by 1 in each year to 1990.[8] Likewise, while voter registration is expected to be more stable than responses to party identification items on surveys, it cannot be assumed that registration is completely immune to the short-term factors associated with elections and, in particular, may reflect differentials in voter registration efforts by parties. To allow for such effects, a dummy variable is constructed that is 1 in presidential election years and 0 otherwise.

ANALYSIS

Figure 1 presents the changes between 1975 and 1990 in Democratic, Republican, and Independent registration among white voters in Louisiana. In 1975, less than 4% of Louisiana voters were registered as Republicans. By 1990, that number had grown to approximately 23%. Likewise, Independent identification increased from approximately 2% to approximately 9% among white registrants. Coincident to the increase in Republican and Independent registrants, the number of Democratic registrants declined not only as a percentage of white registrants but in terms of absolute numbers, as well. In 1975, roughly 1.3 million whites registered as Democrats in Louisiana. By 1990, that number had declined to less that 1.1 million. However, overall white registration increased during this 15-year period, from 1.4 million to slightly less than 1.6 million. These data suggest not only that registrants defected from the Democratic party but also that new white registration disproportionately favored Independents and Republicans. Thus, voter registration among whites in Louisiana tracks reasonably well the declining fortunes of the Democratic party and the rise of Republicans and Independents chronicled by various observers of southern politics.

Is this pattern linked to the level of racial threat in the context? This question is examined at the parish level in Table 1. Since these are pooled time series data (64 parishes over 16 years), a cross-sectionally correlated and timewise autoregressive approach was used to estimate the effect parameters of our model.[9] The ordinary least squares approach to pooled time series employing dummy variables assumes that the cross-sectional units are independent (i.e., homoscedastic) (Stimson 1985). This assumption is unlikely to be true when the pooled units are geographic areas, such as parishes (Kmenta 1986). The approach adopted here not only assumes heteroscedasticity but also assumes mutual correlation of error terms and autoregression.

Table 1, equation 1, presents the effects of the independent variables on the percentage of whites registering as Independent. Consistent with expectations, the year counter is statistically significant, indicating that the level of Independent registration among whites has generally increased over the time period under examination. The model predicts an increase of roughly 6% over the 16 years of the study ($16 * .0038 = .0608$). Likewise, higher median incomes in parishes are associated with higher Independent identification. An increase of $3,500 in median income (the standard deviation for income) yields an estimated increase of roughly 1% in Independent identification. Contrary to expectations, increases in white registration as Independent do not seem to have been influenced by the presence of a presidential election. However, the presence of Jack-

TABLE 1

Effect of the Independent Variables on the Percentage of Whites Registered as Independent, Republican, and Democrat (%)

	INDEPENDENT		REPUBLICAN		DEMOCRATIC	
INDEP. VARIABLES	EQUATION 1	EQUATION 2	EQUATION 3	EQUATION 4	EQUATION 5	EQUATION 6
% black (lagged)[a]	.0126	.1161*	.0907***	.4066***	−.0932***	−.3433***
Year count	.0038***	.0038***	.0107***	.0109***	−.0148***	−.0149***
Election year	.0002	.0002	.0069***	.0069***	−.0072***	−.0072***
Jackson 83/84	.0049***	.0050***	−.0033***	−.0033***	−.0022**	−.0021**
Jackson 87/88	.0033***	.0034***	−.0060***	−.0060***	.0027***	.0027***
Median income (in thousands)	.0032***	.0047***	.0032***	.0083***	−.0067***	−.0112***
Med. Income × % black (lagged)	—	−.0065*	—	−.0214***	—	.0152*
Constant	−.0330**	−.0573**	−.0676***	−.1445***	1.1043	1.1795***
Buse R^2	.597[b]	.593	.854	.861	.853	.864

Note: Unstandardized regression coefficients are estimated using Shazam, Version 6.2. See n. 10 for details.

[a] % black = (the black voting-age population)/(the total voting-age population) lagged one year

[b] Buse R^2 is analogous to the normal R^2 applied to the transformed data.

*.05

**.01

***.001

N = 960 (64 parishes/15 years)

son in a presidential campaign does appear to increase racial sensitivities and result in slightly increased white registration as Independent. Most importantly, however, these data provide no support for the white backlash hypotheses. There appears to be no simple relationship between the black concentration in the parish population and the percentage of whites that register as Independent.

Table 1, equation 3, presents the effects of the independent variables on the percentage of whites registered as Republicans. Consistent with expectations, the level of black concentration within the voting-age population is positively linked to the percentage of white voters in a parish registered as Republicans. An increase of 10% in the black concentration is predicted to result in an increase of approximately 1% in white Republican registration (.10 * .0907 = .009). While this effect is not substantively large, it is consistent with the idea that Republican growth reflects, in part, a response to racial threat within the local context.

Also consistent with expectations, the year counter indicates an increase in Republican registration across the time period. An approximate 1% increment in Republican registration is estimated to occur with the passage of each year. Unlike the case for Independent registration, white registration as Republican increases on average about one-half of 1% in presidential election years. Apparently, the stimulus toward registration in presidential years carries with it a partisan valence. As expected, parishes with higher median incomes tend to have higher percentages of white voters registered as Republicans. It is interesting to note that the effect of this variable on the level of Republican identification is essentially the same as its effect on Independent identification.

The major departure from expectations is the effect of the Jackson campaigns. While Jackson's candidacy is significantly associated with the percentage of whites registering as Republicans, the direction of the effect is opposite to that predicted. The percentage of whites registering as Republicans was expected to be higher during Jackson's campaigns in response to heightened racial sensitivity. However, with the effects of the other variables in the model taken into account, the period of the Jackson campaigns is associated with somewhat lower predicted white Republican registration. It should be emphasized that this does not mean that the level of Republican white registration would be lower in absolute terms but rather that the upward trajectory flattened out somewhat during these periods. Alternatively, one could say that the spike in white Republican registration associated with election years is somewhat attenuated during the elections in which Jackson was a candidate.

Table 1, equation 5 presents the effects of the independent variables on the percentage of whites registered as Democrats. Within rounding, these results are a summation of equations 1 and 3. Equation 5 basically shows the negative effect that nearly all of the independent variables had on the percentage of whites registering as Democrats, while equations 1 and 3 show where those defections went. For Democrats, every passing year yields a further decline in the share of registered whites. This decline is higher in election years and increases with the median income of the white population in the parish. Jackson's first presidential bid accentuated this election year decline, but his second campaign had a positive effect on the Democratic share of white registrants. Again, this does not mean that the actual percentage of whites registering as Democrats increased in this time period. The negative effect of the election year variable is more than twice that of the positive effect of the second Jackson candidacy. Thus the decline in Democratic registration among whites was only somewhat lessened during Jackson's second campaign. While these data are inconclusive as to whether the Jackson candidacy "pushed" whites away from the Democratic party, the negative signs of the Jackson variables in equation 3 make it clear that, at least in the short term, there was no "pull" toward the Republican party as a result of his candidacy.

Based on the power model, we hypothesized that the effects of racial threat would be conditioned by social status, with threat having greater effect among lower-status whites than among higher-status whites. This hypothesis is examined by fitting a multiplicative interaction term between *white median income* and *% black in the population*. The results of this analysis are reported in equation 2 for Independents, equation 4 for Republicans and equation 6 for Democrats. For both Independents and Republicans, the interaction term is negative and statistically significant. Substantively, this result is consistent with the expectations of the power model. The positive effect of racial threat on the growth of Independent and Republican registration is diminished as the median white income of the parish increases. The positive interaction effect in equation 6 means that the decline in white Democratic registration arising with increases in racial threat is attenuated within higher white income contexts.[10]

The model fitted to this point has assumed that the effect of racial threat remained constant during the study period. This assumption is examined in Table 2, where the effects of the percentage of blacks and median income are estimated in biannual cross-sections from 1976 through 1990.[11] Since the remaining variables considered in the original model vary only temporally, their effects cannot be estimated in this analysis. The effects of the percentage of blacks are estimated separately for parishes above and below the mean for white income. This allows the interaction effect between *% black* and *white income* identified in Table 1 to be presented in a more easily interpretable form.

The results in Table 2 for white registration as Independent do not suggest any patterned shift during the study period in the salience of racial threat. For all the cross-sections, the effect of the percentage of blacks on the percentage of whites registering as Independent is not statistically significant. Using pooled time series in Table 1, the effect of the percentage of blacks on the percentage of whites registering as Independent appeared to be conditioned by white income. This is reflected in the relatively stronger effect that percentage of blacks has on independent registration among lower-income parishes than among higher-income parishes in Table 2. These differences are simply not of sufficient magnitude to obtain statistical significance when decomposed into cross sections.

In contrast, the results in Table 2 for the effect of the percentage of blacks on the percentage of whites registered as Republicans illustrate the conditioning influence of white income and also reveal a patterned shift in the salience of racial threat. First, consistent with the findings in Table 1 and with the expectations of the power hypothesis for every cross section, the effect of the percentage of blacks is higher in low-white-income parishes than in high-white-income parishes. Indeed, in not one cross section does the effect of racial concentration in the voting-eligible population achieve statistical significance in the high-income parishes. Second, while the effects of the percentage of blacks within lower-income parishes are only statistically significant beginning with the 1982 cross section, from 1976 through 1990, the magnitude of the effects of the percentage of blacks on the percentage of Republicans increases. Indeed, between 1980 and 1990, the effect of the percentage of blacks on the percentage of Republicans almost triples. In 1980, a 10-point increase in the percentage of the voting-eligible population who were black is predicted to increase the percentage of whites registered as Republicans by approximately .85 percentage points. In 1990, that same increase is linked to a growth of 2.26 points in the percentage of white Republicans. As required, the results for Democratic registration are the mirror image of those for Republican registration. Thus, during the study period, racial threat to white political control increasingly became an important consideration in the growth of Republican registration and the decline of Democratic registration among whites in Louisiana.

DISCUSSION

The issue of a linkage between the rise of black influence within the Democratic party and the decline of Democratic identification among whites has been debated in both the popular media and the scholarly press. The presence of such a linkage is consistent with power theory, which views relations among groups as a function of their competitive positions in political, economic, and social arenas. Generally, this approach predicts that the willingness of a dominant group to remain in a particular context declines as its hegemony is threatened by increases in the power of the nondominant group. Thus, as blacks constitute a larger element of the Democratic coalition in a particular context, the power approach predicts that whites will increasingly exit the party. Moreover, this approach suggests that the response among whites will be conditioned by their vulnerability to competition from blacks. Thus, lower-status whites are predicted to be more sensitive than are higher-status whites to the threat posed by higher black concentrations.

The results of the present study provide support

TABLE 2

Cross-sectional Examination of the Interactive Effects of the Percentage of Blacks and Median Income on Partisan Registration

DEPENDENT VARIABLES YEAR AND PARTY	COEFFICIENTS FOR INDEPENDENT VARIABLES			
	PERCENT BLACK IN LOWER INCOME PARISHES	PERCENT BLACK IN HIGHER INCOME PARISHES	MEDIAN INCOME	R-SQUARE
1976				
Republican	.0086	−.0258	.0034**	.13
Independent	.0101	−.0178	.0023**	.13
Democrat	−.0189	.0437	−.0057**	.14
1978				
Republican	.0115	−.0335	.0040**	.14
Independent	.0134	−.0125	.0028**	.14
Democrat	−.0249	.0460	−.0068**	.15
1980				
Republican	.0850#	−.0085	.0057**	.12
Independent	.0437	.0079	.0047***	.23
Democrat	−.1287#	.0007	−.0103**	.17
1982				
Republican	.1109*	.0064	.0063**	.14
Independent	.0328	.0108	.0049***	.26
Democrat	−.1436	−.0172	−.0111**	.18
1984				
Republican	.1546**	.0065	.0091**	.18
Independent	.0298	.0088	.0058***	.29
Democrat	−.1844*	−.0153	−.0149***	.22
1986				
Republican	.1810**	.0018	.0106**	.19
Independent	.0280	.0021	.0056***	.19
Democrat	−.2090*	−.0039	−.0162***	.22
1988				
Republican	.2086**	.0111	.0115**	.19
Independent	.0263	.0016	.0049***	.24
Democrat	−.2349*	−.0127	−.0165***	.21
1990				
Republican	.2264**	.0144	.0119**	.19
Independent	.0294	.0048	.0046**	.23
Democrat	−.2558**	−.0192	−.0165***	.20

Note: The ordinary least squares regression coefficients are derived from fitting the following model to the dependent variables:

$$\%REP \text{ or } \%IND \text{ or } \%DEM = a + b_1 \text{ (median income)} + b_2 \text{ (L} \times \%\text{black)} + b_3 \text{ (H} \times \%\text{black)}$$

where L = 1 where the parish median income is below average median income, 0 otherwise, and H = 1 where the parish median income is above average median income, 0 otherwise. This allows the effect of the percentage of blacks to be estimated separately for parishes above and below the average median income. There are 29 parishes in the Higher Income group and 35 in the lower.

#$p \le .10$.

*$p \le .05$.

**$p \le .01$.

***$p \le .001$.

N = 64 Parishes per year

for the operation of a linkage between increases in black threat and growth in Republican registration. Between 1976 and 1990, Republican registration grew dramatically in Louisiana. At the parish level this increase was found to be related to the black concentration within the voting-age population. The larger the percentage that blacks constituted of the voting-age population in a parish, the larger the increase in the percentage of whites registering as Republican. Moreover, this linkage increased steadily in magnitude during the study period. In sum, these data suggest that the increasing influence of blacks within the Democratic coalition was a factor in the growth of Republican registration in Louisiana and that its importance increased through the 1980s.

The impact of racial threat was also found to be conditioned by white social status as predicted by power theory. Indeed, the relationship identified between black threat and growth in Republican registration was confined almost entirely among parishes with lower white median incomes. Among parishes with higher white median incomes, the level of black concentration appears to be unrelated to growth in Republican registration. These results, combined with the positive relationship between median white income and Republican registration, suggest that Republican growth is benefiting from a complex relationship between race and class. Republicans are gaining registration among both higher- and lower-status whites. Among higher-status whites, this shift toward Republican registration probably reflects traditional conservative issue appeals. Among lower-status whites, the shift toward Republican registration appears to reflect concerns about black influence within the Democratic party. Consistent with the argument of Huckfeldt and Kohfeld (1989), the end result of the operation of racial threat among lower-status whites is that the class cleavage between the Democratic and Republican parties is diminished.

These concerns of the lower-status whites should not be interpreted simply as racism, although the willingness of a large number of white Louisiana voters to support David Duke in his campaign for governor suggests that the operation of racism cannot be dismissed. Available evidence indicates that, in fact, blacks have become a larger component of the Democratic coalition, Democratic politicians at both the federal (Whitby, 1987; Whitby and Gilliam, 1991)[12] and state (Herring, 1990) levels have become more sensitive to black political demands. Thus, whites who exit the Democratic party in response to its increasingly black makeup may be seen as exiting an organization that in reality is less sensitive to their interests.

While providing clear support for the operation of racial threat in the decline of white support for the Democratic party, the results of this analysis also indicate that other factors played a role in this decline. Across the time period examined, the growth of Independent registration was found to be unrelated to the level of black concentration within the Democratic party. Thus, the movement of Democratic registrants into the Independent category does not appear to have been linked to the threat of black influence within the Democratic party. Moreover, the significant effects of the trend and the election-year variables indicate that general forces outside of the parish context were at work in stimulating growth in Republican and Independent registration. The magnitude of the effects for these variables indicates that at least on average, their effects were more important than those of racial threat. However, it should be remembered that registration data provide a conservative test of the threat hypothesis and may well underestimate the effects of racial concentration.

TABLE A-1

Descriptive Statistics for Principal Variables (N = 64)

VARIABLE	MEAN	S.D.	MINI-MUM	MAXI-MUM
1975				
% black	.28	.13	.04	.62
% white Republicans	.02	.02	.00	.11
% white Independents	.01	.01	.00	.05
% white Democrats	.96	.03	.86	1.00
1980				
% black	.27	.13	.03	.59
% white Republicans	.06	.04	.02	.19
% white Independents	.05	.03	.01	.11
% white Democrats	.88	.07	.72	.98
1985				
% black	.28	.13	.04	.58
% white Republicans	.12	.06	.03	.28
% white Independents	.07	.03	.02	.14
% white Democrats	.81	.09	.59	.95
1990				
% black	.28	.13	.04	.59
% white Republicans	.18	.08	.07	.36
% white Independents	.08	.03	.02	.14
% white Democrats	.74	.10	.50	.90

APPENDIX

Table A-1 shows descriptive statistics for selected years for the dependent variables and the percentage of the population that is black.

Table A-2 shows the correlations among the principal dependent and independent variables employed in the analysis.

TABLE A-2

Correlation Matrix for Pooled Data (N = 960)

	WHITE INC.	% BLACK	% WHITE REP.	% WHITE IND.	% WHITE DEM.	YEARCOUNT
White income	1.0000	—	—	—	—	—
% black	−.02650	1.0000	—	—	—	—
% white Republicans	.17462	.11889	1.0000	—	—	—
% white Independents	.36857	.04975	.78275	1.0000	—	—
% white Democrats	−.24895	−.10163	−.97772	−.89596	1.0000	—
Yearcounter	.00000	−.00110	.67003	.54735	−.66281	1.0000

Notes

We would like to acknowledge the assistance of our colleagues Alan Abramowitz, Merle Black, and Courtney Brown, who commented on earlier versions of this manuscript.

1. These authors vary in the importance which they would attach to racial antagonism in producing defection. For

example, Black and Black see white defection as a complex process of which racial antagonism is only one component, a view with which we agree (1987, 271).

2. The power approach has also been referred to as the "conflict approach" or "competitive ethnicity" (see Blalock 1967; Blumer 1958; Schermerhorn 1956; Wilson 1973).

3. Louisiana provided total registration by race and by party for each parish prior to 1975, but party registration was not provided by race. Georgia, North Carolina, and South Carolina provide voter registration data by race, but it is not broken down by party.

4. Registration reports are actually available on a quarterly basis, although only the final quarter reports issued in December of each year are employed in this analysis. Florida provides voter registration reports by race and party every two years during primary and general elections.

5. The effects of the percentage of blacks in the population on the percentage of eligible whites who registered was estimated controlling for (1) white median income, (2) whether Jesse Jackson was campaigning, (3) whether it was an election year, and (4) a trend counter for time. The coefficient for the percentage of blacks was .1315, which was statistically significant at .05.

6. The percentage of blacks within the Democratic party is BD/(BD + WD), where BD equals the number of black Democrats and WD equals the number of white Democrats. The dependent variable is the percentage of whites who are Republican, or WR/(WD + WR + WI), where WR equals the number of white Republicans and WI equals the number of white independents. As WD declines, ceteris paribus, the percentage of blacks in the Democratic party must increase.

7. The analysis was replicated using the percentage of registered Democrats in each parish who are black as the measure of racial threat. While the magnitudes of coefficients varied, the overall pattern of results was not substantially different from that reported here using the percentage of blacks in the population.

8. The trend counter as a surrogate for a general pattern over time has been used by the Bureau of Labor Statistics and econometricians alike (see Maddala 1977, 344–46).

9. The estimation techniques used assume that we have a cross-sectionally correlated and timewise autoregressive model. We therefore assume that the cross sections (the parishes) are not independent over time. The solution to this problem has been suggested by Kmenta (1971, 512–14). This model assumes that there is heteroscedasticity, mutual correlation, and autoregression. The procedure to attain efficient estimates involves transforming the variance–covariance matrix with respect to parishes, over time. This has a similar effect to removing the mean over-time value of the variables from each parish.

This is done as a multistep procedure that first involves transforming the residuals with respect to time. This removes the problem of autoregressive behavior. These residuals are then used in an ordinary least squares regression to calculate the regression residuals. The regression residuals are then transformed to remove cross-sectional correlation and create consistent estimates for variance and covariance. These estimates are then used in calculating the parameter estimates.

10. The significant interaction between *median income* and *% black* might be serving simply as a proxy for a nonlinear relationship between median income and the dependent variables. However, as seen in the Appendix, white median income and the percentage of blacks are not statistically related in these data, making this unlikely. Moreover, when *median income squared* (a nonlinear term) was added to equations 2, 4, 6, its coefficient was not statistically significant, while the interaction term between *median income* and *% black* remained statistically significant. Similar results were obtained when *% black squared* was added to these equations.

11. In addition to the analysis in Table 2, a multiplicative interaction term between the year counter and *% black* was added to equations 1, 3, and 5 (Table 1). This term was statistically significant for Republican and Democratic registration but not for Independent registration. While patterns appearing in a pooled cross section may not appear in an analysis of the component cross sections or may only be approximated (Stimson 1985), the results in Table 2 appear to capture the patterns of the pool cross section reasonably well.

12. Some studies have found no relationship between the racial concentration in the district and liberal voting. Whitby and Gilliam (1991) provide evidence that racial composition did have such an effect in a transitional period but that by the 1980s the contribution of black voters to the party extended their influence beyond specific contexts, with the result being a generally greater liberalness among southern Democrats not correlated to racial concentration.

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American Political Science Review Vol. 88, No. 2 June 1994

THE STRATEGIC ROLE OF PARTY IDEOLOGY WHEN VOTERS ARE UNCERTAIN ABOUT HOW THE ECONOMY WORKS

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A continuum of voters, indexed by income, have preferences over economic outcomes. *Two political parties each represent the interests of given constituencies of voters: the rich and the poor. Parties/candidates put forth* policies*—for instance, tax policy, where taxes finance a public good. Voters are uncertain about the* theory of the economy, *the function that maps policies into economic outcomes. Parties argue, as well, for theories of the economy. Each voter has a prior probability distribution over possible theories of the economy; after parties announce their theories of the economy, each voter constructs an a posteriori distribution over such theories. Suppose that voters are unsure how efficiently the government converts tax revenues into the public good. Under reasonable assumptions the party representing the rich argues that the government is very inefficient and the party representing the poor argues the opposite. What appear as liberal and conservative ideological views emerge as simply good strategies in the electoral game.*

In the classical voting model, candidates compete for voters by proposing policies; each voter has a preference ordering over policies and votes for the candidate who proposes the policy preferred. I shall suggest that electoral competition between candidates takes the form of trying to influence voters' *beliefs* concerning the *efficacy of different policies*. I shall remain classical in assuming that voters' preferences over *economic outcomes* are fixed.[1] Consider this model. Each voter has a preference ordering over economic outcomes—for instance, over the size of his own income and of public goods provided by the government in an outcome. Candidates can announce not outcomes but only *policies* as part of their platforms. A voter must have a *theory of the economy*, which associates to any policy an economic outcome in order to decide between policies.[2] Voters are uncertain about what the true theory of the economy is. Candidates compete by announcing not only a policy but also a theory of the economy. By announcing (and arguing for) different theories of the economy, candidates attempt to influence the beliefs of voters in such a way that the candidates' goals are met.

For example, suppose there are two political parties, which represent the interests of two income groups in the population: the poor and the rich. The electoral issue is tax policy: incomes will be taxed to finance a public good. The public good is in large part an income transfer from the rich to the poor. The economic parameter about which voters are uncertain is the effort-supply elasticity of workers with respect to the tax rate.[3] (Thus, tax rates will affect labor supply, which in turn will affect the amount of tax revenue raised and of public good provided.) The party of the rich wants as little of the public good as possible, and the party of the poor, the opposite. A theory of the economy consists in a function specifying the effort-supply elasticity schedule for the population as a function of the tax rate. In actual politics, we observe that the rich party usually proposes a low tax rate and a theory saying the effort-supply elasticities are negative and large in absolute value, while the poor party proposes a high tax rate and a theory saying the effort supply is tax-inelastic.

I shall show that these different views of how the economy works (i.e., different "theories of the economy") will be put forth in electoral equilibrium by parties whose *sole principle* is to maximize the expected utility of a particular income class—that is to say, these apparently ideological views emerge for reasons that are entirely strategic and have nothing to do with principled disagreements concerning how the economy works. (For example, the penultimate model shows that even if the right and left parties agree about what the effort-supply elasticity of citizens is, in an electoral equilibrium with malleable voters, the right party will propose a theory saying that effort-supply elasticities are large and negative, while the left party will say that effort supply is relatively inelastic with respect to the tax rate.)[4] This result implies that based on the observation that parties appear to have principled differences about how the economy works we cannot reject the hypothesis that they are purely *class parties*, in the sense that they have no ideological preconceptions except to pursue particular class interests. The theory of the economy announced by a party may be purely a strategic matter.

Suppose there are two parties, each representing a different segment of the population (e.g., the rich and the poor). These parties have policy preferences, so that the policy outcome of an election (not just whether it wins) matters to the party. Suppose that voters are uncertain about how the economy works and that they are influenced in their beliefs, to varying extents, by announcements that candidates make about how the economy works. Each candidate (or party) announces, as its strategy in the voting game, a *policy* and a *theory of the economy*. Can we characterize the policies and theories of the economy that parties put forth in equilibrium?

The basic elements needed to formalize this model are as follows. I assume there is a continuum of voters, indexed by s in a sample space S, and distributed according to a probability measure F on S. (Thus, for a measurable subset S^* of S, $F(S^*)$ is the fraction of the population that lies in S^*.) In the particular examples that I shall study, s is the real wage that a voter earns, or the voter's "skill level." There is a set B of possible policies, with generic element denoted b: these will be income tax rates. There is a sample space T of theories of the economy, with generic element t. A voter s is characterized by a family of preference orderings $v(\cdot, s, t)$ on B and a measure $\Phi(\cdot, s)$ on T, with density $\varphi(t, s)$. Agent s's preference ordering over policies is $v(\cdot, s, t)$ if the true theory of the economy is t. Think of v as an indirect utility function, derived as follows. For any policy b there is an economic outcome that will result if the true theory of the economy is t and v records voter s's utility in that outcome. The simplest case to study is the one where B, T, and S are intervals of real numbers and for each t and s, the preferences $v(\cdot, s, t)$ are single-peaked: this, indeed, will be the case in the examples. The probability measure $\Phi(\cdot, s)$ summarizes voter s's prior beliefs concerning the theory of the economy; that is, for a measurable subset T^* of T, $\Phi(T^*, s)$ is the probability that s assigns to the event that the true theory of the economy is some t in T^*.

There are two political parties, each of which represents diligently the interests of a constituency of voters. (I depart from the more common assumption that parties/candidates are Downsian, that is, are simply interested in winning elections. There is a literature where candidates/parties have preferences over policies; see, e.g. Alesina 1988; Calvert 1985; Hansson and Stuart 1984; Roemer 1992, 1993; Wittman 1977, 1990.) I shall assume that the parties have *representative constituents* s_1 and s_2. These two voters might be the median voters in larger coalitions of voters, each of which is a party's constituency. A party's goal is to maximize the (expected) utility of its representative constituent.[5]

I must assume something about the beliefs of parties concerning the theory of the economy: I think the most interesting case to examine is one in which both parties know (or believe they know) the true theory and agree on what that theory is (call it t_0). I shall assume this to be the case.[6]

Parties can influence the beliefs of voters when they propose theories of the economy. Under the assumption just made, they dissemble when they propose theories other than t_0. Party i's *platform* consists of a policy–theory pair, (b^i, t^i). The essential part of the model is the description of the effects that the proposed party platforms have on the beliefs of voters concerning the true theory of the economy. I shall suppose that when a voter is presented with two theories t^1 and t^2 by the parties, the voter's prior beliefs $\Phi(\cdot, s)$ become altered into posterior beliefs, $\Phi^{\text{ex}}(\cdot, s)$. That is to say, there is a mapping Ψ that associates to any quadruple (t^1, t^2, φ, s), where $t^1, t^2 \in T$, φ is a probability density on T, and s is a voter, a probability density φ^{ex} on T. Ψ is a primitive datum in this model: it describes the effect of the parties' announcements of a pair of theories of the economy on a voter's beliefs concerning the true theory.

In recent work, Gerber and Lupia have made Ψ endogenous, assuming that voters are rational (Gerber and Lupia 1992; Lupia 1992). They study how voters use information about candidate attributes to decide whether to believe announcements that candidates make about the effects of policies. Similarly, Austen-Smith (1990) models a rational inference process of legislators who try to evaluate the information that other legislators put forth in debate about the efficacy of their proposed policies. I take the inference process summarized by Ψ as a datum but restrict Ψ in only a fairly mild way.

As an example, consider the mapping Ψ that acts as follows. Let T be an interval of real numbers. If the parties propose theories t^1 and t^2, then any voter s with prior Φ revises the probability distribution by assigning probability zero to the event $T \setminus [t^1, t^2]$ and renormalizes the prior on the interval $[t^1, t^2]$. Formally, if $t^1 < t^2$, then for all s,

$$\Psi(t^1, t^2, \varphi, s) = \begin{cases} 0, & \text{if } t \text{ is not in } [t^1, t^2] \\ \varphi(t) \Big/ \displaystyle\int_{t^1}^{t^2} \varphi(t)\, dt, & \text{if } t \in [t^1, t^2]. \end{cases} \quad (1)$$

If $t^1 = t^2$, then the posterior measure is the unit mass at the common value t^1. Let us denote the posterior probability measure on T of voter s, when faced with the parties' theory proposals t^i, $\Phi^{\text{ex}}(\cdot, s; t^1, t^2)$. The associated density function, if there is one, is $\varphi^{\text{ex}}(t, s; t^1, t^2)$. In words, under the inference process described by Ψ, every voter concludes that any theory of the economy t not in the interval $[t_1, t_2]$ has probability zero of being true and simply renormalizes the old density function on the new support $[t_1, t_2]$. I do not assert that such an inference process is reasonable; this Ψ is given only as an example to clarify the idea.

I shall make only one assumption about this inference process:

Assumption 1. (Monotonicity). *The mean of* $\varphi^{ex}(t, s; t^1, t^2)$ *is a nondecreasing function of* t^i, *for* $i = 1, 2$.

Assumption 1 says that if either party *increases* the parameter that it announces characterizing the theory of the economy, then no voter will *decrease* his or her mean belief about the value of that parameter. Assumption 1, in particular, permits voters *not to change their beliefs* about the unknown parameter when parties change their announcements about its value. Thus, a poor voter can ignore the announcement that the rich party makes: this is permitted by assumption 1. The reader can verify that the inference process Ψ of equation 1 satisfies assumption 1.

A party's objective, as I said, is to maximize the expected utility of its representative constituent. To formulate this objective, we must first describe how to calculate the probability that a party wins the election, given the two platforms (b^i, t^i), $i = 1, 2$. Given a posterior distribution Φ^{ex}, a voter s evaluates a policy b according to its expected utility, $\int_T v(b, s, t)\varphi^{ex}(t, s; t^1, t^2)\, dt$. Thus, we define the set of voters who prefer b^1 and b^2 when facing the platforms (b^i, t^i), $i = 1, 2$ as

$$A(b^1, t^1, b^2, t^2) \equiv \left\{ s \middle| \int_T v(b^1, s, t)\varphi^{ex}(t, s; t^1, t^2)\, dt > \int_T v(b^2, s, t)\varphi^{ex}(t, s; t^1, t^2)\, dt \right\}.$$

Policy b^1 defeats b^2 if and only if $F(A(b^1, t^1, b^2, t^2)) > 1/2$—that is, when the mass of voters who prefer one platform to another constitutes a majority of the population. We may therefore define the function $P(b^1, t^1, b^2, t^2)$—the probability that b^1 defeats b^2 given the two platforms t^1 and t^2—as

$$P(b^1, t^1, b^2, t^2) = \begin{cases} 0 & \text{if} \quad F(A(b^1, t^1, b^2, t^2)) < \frac{1}{2} \\ \frac{1}{2} & \text{if} \quad F(A(b^1, t^1, b^2, t^2)) = \frac{1}{2} \\ 1 & \text{if} \quad F(A(b^1, t^1, b^2, t^2)) > \frac{1}{2}. \end{cases}$$

I have assumed that parties attempt to maximize the expected utility of their representative constituent, where parties evaluate expected utility according to what they believe to be the true theory of the economy, t_0. Thus, the *payoff* to party i, when the platforms $\{(b^1, t^1), (b^2, t^2)\}$ have been proposed is defined as

$$\Pi((b^1, t^1), (b^2, t^2), i) = P(b^1, t^1, b^2, t^2)v(b^1, s_i, t_0) + (1 - P(b^1, t^1, b^2, t^2))v(b^2, s_i, t_0).$$

This just says that the payoff to the party representing s_i when the parties propose platforms (b^1, t^1) and (b^2, t^2) is

(Probability 1st Party Wins

× Actual Utility to Voter s_i under party 1's Policy)

+ (Probability 2d Party Wins

× Actual Utility to Voter s_i under Party 2's Policy).

By *actual utility,* I mean the utility that the voter gets under what parties think is the true theory (t_0) of the economy.

I am now ready to define an electoral equilibrium for this model, as follows:

DEFINITION 1. *For an environment* $(F, S, T, B, v, \{\Phi(\cdot, s)\}s \in S, \Psi, s_1, s_2, t_0)$ *an* electoral equilibrium with malleable voters *is a pair of platforms* $(\overline{b^1}, \overline{t^1}), (\overline{b^2}, \overline{t^2})$ *such that each platform is a best response to the other party's platform; that is,*

$$(\overline{b^1}, \overline{t^1}) \in argmax\ \Pi(b^1, t^1), (\overline{b^2}, \overline{t^2}), 1)$$

and

$$(\overline{b^2}, \overline{t^2}) \in argmax\ \Pi((\overline{b^1}, \overline{t^1}), (b^2, t^2), 2).$$

That is, each party's platform is a best response to the platform of the other party, where each party's payoff function is the expected utility of its representative constituent under the true theory of the economy. This equilibrium notion is a variant of Bayesian Nash equilibrium.

I must emphasize that in this model, the parties know all the data specifying the politicoeconomic environment, so that *there is no uncertainty concerning the outcome of the election,* except in the case when the probability of victory at equilibrium is just 1/2. (In particular, although voters are uncertain about how the economy works, parties know exactly what voter's beliefs are and hence can calculate what the outcome of the elections will be.) The model is therefore too simple to reflect a reality in which the outcomes of elections are uncertain: that uncertainty would obtain only if parties did not know everything about voters' beliefs and preferences. I have elected to assume that parties know everything about voters in order not to bring in an issue that would complicate the model a great deal, namely, party uncertainty about voters' beliefs or preferences. (Readers who are interested in seeing a model in which parties are uncertain about voter preferences might consult Roemer 1993.)

A MEDIAN VOTER THEOREM

There is a median voter result for this model: at an electoral equilibrium with malleable voters, both parties will propose the same policy, although they will generally propose different theories of the economy. I emphasize that the first fact is due to the assumption that parties have complete information about voters' preferences and beliefs. If parties had incomplete information about voters' characteristics, they would announce different policies, as well, in equilibrium. The general fact is that if parties both (1) have policy preferences and (2) are uncertain about voter preferences or beliefs, then the electoral equilibrium involves the parties putting forth different policies (see Roemer 1993). If one introduces only one of these two assumptions into the classical Downsian model, however, a median voter equilibrium still attains. (For a proof of this claim, see Roemer 1994.) The present model assumes condition 1 but not condition 2, for reasons of simplicity only.

I shall, for the moment, fix the theories $(\overline{t^1}, \overline{t^2})$ arbitrarily and define what the equilibrium concept would be if parties competed only with policies as strategies in the electoral game. The expected utility of voter s, given the proposals for theories of the economy $(\overline{t^1}, \overline{t^2})$ is

$$\tilde{v}(b, s) = \int_T v(b, s, t)\varphi^{ex}(t, s; \overline{t^1}, \overline{t^2})\, dt.$$

I now consider the electoral game between two parties (who complete only over policies) for voters whose preferences over policies are given by the functions $\tilde{v}(b, s)$. The probability that a policy b^1 defeats b^2 is given by

$$\tilde{P}(b^1, b^2) = \begin{cases} 0 & \text{if} \quad F(A(b^1, \overline{t^1}, b^2, \overline{t^2})) < \frac{1}{2} \\ \frac{1}{2} & \text{if} \quad F(A(b^1, \overline{t^1}, b^2, \overline{t^2})) = \frac{1}{2} \\ 1 & \text{if} \quad F(A(b^1, \overline{t^1}, b^2, \overline{t^2})) > \frac{1}{2}. \end{cases} \tag{2}$$

The payoff function for party i is given by

$$\tilde{\Pi}(b^1, b^2, i) = \tilde{P}(b^1, b^2)v(b^1, s_i, t_0) + (1 - \tilde{P}(b^1, b^2))v(b^2, s_i, t_0). \tag{3}$$

We have the ingredients to define an electoral equilibrium of this "nested" model as follows:[8]

Definition 2. *Fix* $(\overline{t^1}, \overline{t^2})$. *For an environment* $(F, \tilde{v}, v, B, S, \Phi^{ex}, s_1, s_2, t_0)$, *a* partial electoral equilibrium *at* $(\overline{t^1}, \overline{t^2})$ *is a pair of platforms* $(\overline{b^1}, \overline{b^2})$ *such that each platform is a best response to the other party's platform; that is,*

$$\overline{b^1} \in \operatorname{argmax} \tilde{\Pi}(b^1, \overline{b^2}, 1)$$

and

$$\overline{b^2} \in \operatorname{argmax} \tilde{\Pi}(\overline{b^1}, b^2, 2).$$

I now make four assumptions:

Assumption 2. $\tilde{v}(b, s)$ *is continuous and strictly concave in* b.[9]

Assumption 3. $\tilde{v}(b, s)$ *is continuous in* s.

Assumption 4. $\hat{b}(s) = \operatorname{argmax} \tilde{v}(b, s)$ *is strictly monotone decreasing in* s.

Assumption 5. F *is absolutely continuous with respect to Lebesgue measure.*

We have, under these assumptions, the classical median voter result:

Lemma 1. (Classical Median Voter Theorem). *Let assumptions 1–4 hold. Then* $\hat{b}(s^m)$, *where* s^m *is the median of* F, *defeats all other policies by majority vote.*

(For a proof, see Roemer 1994, lemma 2.1. Note that lemma 1 does not assert that the parties put forth $\hat{b}(s^m)$ as their policies in equilibrium.)

I next make a further assumption:

Assumption 6. *The function* $\tilde{v}$ *has a* single crossing property *on* $B \times S$, *i.e., for any distinct pair* $b^1, b^2 \in B$, *there is at most one* s *such that* $\tilde{v}(b^1, s) = \tilde{v}(b^2, s)$.

Next, define

$$\underline{b} = \operatorname*{argmax}_b v(b, s_1, t_0).$$

$$\bar{b} = \operatorname*{argmax}_b v(b, s_2, t_0).$$

I shall abbreviate $\hat{b}(s^m)$ as $\hat{b}^m$. $\bar{b}$ and $\underline{b}$ are the policies that maximize the "true interests" of constituents s_2 and s_1, respectively, while $\hat{b}^m$ is the perceived favorite policy of the median voter, given his probability density φ^{ex} over theories of the economy. We can now state

Theorem 1. *Fix* $(\overline{t_1}, \overline{t_2})$ *and let assumptions 2–6 hold. Let* $s_1 < s^m < s_2$. *If* $\bar{b} < \hat{b}^m < b$, *then* $(\hat{b}^m, \hat{b}^m)$ *is the unique partial electoral equilibrium at* $(\overline{t_1}, \overline{t_2})$.[10]

(For proof, see the Appendix.) Theorem 1 asserts that once the parties have decided upon the theories of the economy they shall propound, then both parties announce the median voter's ideal policy in the unique electoral equilibrium. I reiterate that theorem 1 is not the classical Downsian result; for in this article, parties are not trying to maximize the probability of getting elected, as Downsian parties are: they are trying to maximize the expected utility of their constituents. It is this different objective that requires us to employ an assumption like the single crossing property to get the median voter result.

Note that any *full* electoral equilibrium with malleable voters $\{(\overline{b^1}, \overline{t^1}), (\overline{b^2}, \overline{t^2})\}$ is a *partial* electoral equilibrium at $(\overline{t^1}, \overline{t^2})$. We can therefore use theorem 1 to study the nature of electoral equilibrium with malleable voters in the examples that follow.

Finally, I remark that the statement of theorem 1 remains unchanged if we drop the assumption that the two parties agree on what the true theory of the economy is. If party i believes the true theory is t_i^*, we need only redefine $\underline{b}$, $\bar{b}$, and $\tilde{\Pi}(\cdot, \cdot, i)$ in the appropriate way, and the proof of theorem 1 as stated remains the same.

PARTY COMPETITION OVER THE EFFICIENCY OF GOVERNMENT

I shall specify economic environments where I can be explicit about the uncertainty that voters have about how the economy works and can therefore study explicitly the nature of the platforms that parties will put forth in the equilibrium of definition 2. The result that will be used to analyze these political economies is theorem 1.

I consider an economy in which voters differ only with respect to their "skill" (or the real wage they earn) and their prior beliefs concerning the efficiency of government. The voters decide the uniform rate at which income is to be taxed. Taxes finance a public good. What voters are uncertain about is the efficiency with which the government converts tax revenue into the public good. A *theory of the economy* consists of a value of this conversion parameter.

Let there be a continuum of voters, indexed by real-wage levels $s \in [\underline{s}, \bar{s}] = S$, where s is distributed according to a probability density function $f(s)$ on $[\underline{s}, \bar{s}]$. Each voter has preferences given by

$$u(y, G, L) = y + G - \frac{d}{2} L^2, \quad (4)$$

where y is consumption of the private good (the real-wage good), G is consumption of the public good, and L is labor expended. If B is total tax revenue, then the government's *production function* is

$$tB = G,$$

where t is the *efficiency of government* (the conversion factor of taxes into the public good). Voters of type s have prior beliefs about the value of t specified by a probability density function $\varphi(t; s)$ on the sample space $T = [\underline{t}, \bar{t}]$, some real interval.

Given proposals t^1 and t^2 for the theory of economy by the two parties and a voter with a prior $\varphi(t)$, the posteriori beliefs of the voter are given by the density function

$$\varphi^{\text{ex}}(t, s; t^1, t^2) = \Psi(t^1, t^2, \varphi, s),$$

where Ψ is the voter's inference process.

Assumption 1 is central to the argument that follows; it can be justified in either of two ways. The first would attempt to deduce that assumption 1 characterizes the behavior of rational voters, who, using the information they have about candidates/parties, update their beliefs about how the economy works after parties' announcements are made. (This is the spirit of Lupia 1992 and Austen-Smith 1990.)[11] The second would be to say that voters are not fully sophisticated (i.e., rational) in the sense that they do not take full account of the fact that parties are behaving strategically in announcing theories of the economy. Hence, they are influenced by what parties say when, perhaps, they should not be.

As I have said, I do not here propose a complete theory of the process of voter inference. Nevertheless, I believe that assumption 1 is a plausible restriction on that inference process when voters are confronted with politicians who announce theories of how the economy works. Notice that what assumption 1 does is to compare a voter's beliefs about how the economy works when the pair of announcements of theories by the candidates changes from (t^1, t^2) to, say, $t^1, t^{2*})$. Assumption 1 says that if $t^{2*} > t^2$, then no voter would revise his mean belief about the true value of t downward (from what he would have believed if the announcements had been (t^1, t^2)). If such a change were to occur in party 2's announcement in real politics, then the "experts" associated with party 2 and the media who support party 2 would argue that t^{2*} is the true theory of the economy. The experts and media supporting party 1 would continue to argue that t^1 is the true theory. Thus, the sources of information to which the voter is exposed would maintain, on average, that the true value of t is higher than if party announcements had been the pair (t^1, t^2). It is not unreasonable to suppose that this influences voter beliefs about t in an upward direction—or, more precisely, as assumption 1 says, no voter would revise a belief about t downward in the face of this change in party 2's announcement.

I next describe the concept of *economic equilibrium* at a tax rate b for voters with posteriori beliefs given by $\varphi^{\text{ex}}(t; s)$.

DEFINITION 3. *An* economic equilibrium *at tax rate* b *for voters with posteriori beliefs* $\varphi^{ex}(t; s)$ *concerning the theory of the economy and with preferences given by equation 4 is a function* $G(\cdot, b) : T \to \Re_+$ *and a function* $L(\cdot, b) : S \to \Re_+$ *such that*

1. $$L(s, b) = \underset{L}{\operatorname{argmax}} \int_T ((1 - b)sL + G(t, b) - \frac{d}{2} L^2)\varphi^{ex}(t; s)\, dt \text{ for all } s \in S;$$

2. $$t \int_S bsL(s, b)f(s)\, ds = G(t, b) \text{ for all } t \in T.$$

This first expression says that $L(s, b)$ is the optimal labor supply for a type s agent with preferences given by equation 4 who believes that $G(t, b)$ will be the value of the public good if t is the true theory of the economy. The agent in the expression is choosing labor supply to maximize expected utility, with the expectation taken over the probability distribution that expresses the agent's beliefs about what the true theory of the economy is.

The second expression says that if, for all s, agent s supplies labor $L(s, b)$ and the true efficiency of the government is t, then, indeed, $G(t, b)$ will be the value of the public good.

The equilibrium of definition 3 is a rational expectations equilibrium, in the sense that the labor supplied by agents—given (1) their posteriori beliefs about how the economy works, (2) that the tax rate is b, and (3) their projections about the amount of the public good that will be supplied under the various possible values of the efficiency of government that voters believe possible—will indeed generate precisely the levels of government spending that they believe to be forthcoming at possible values of the efficiency of government. Thus, in such an equilibrium, *no agent has good reason to revise beliefs*. To put it another way, there is no reason for any voter to update beliefs about the efficiency of government in such an equilibrium.

I shall show that for any $b \in [0, 1]$, there is a unique economic equilibrium à la definition 3. Thus a voter evaluates his *utility at tax rate b* as his (expected) utility in that equilibrium, defined as

$$v(b, s; t^1, t^2) = (1 - b)sL(s, b) + \int_T G(t, b)\varphi^{\text{ex}}(t; s, t^1, t^2)\, dt - \frac{d}{2} L(s, b)^2. \quad (5)$$

I include (t^1, t^2) as arguments of v to remind us that the economic equilibrium is calculated for given posteriori beliefs φ^{ex}, which are induced by particular party proposals (t^1, t^2).

I can now apply theorem 1 to characterize political economic equilibrium in the present model. The parties of the left and the right represent given voters $s_1 < s_m$ and $s_2 > s_m$, respectively, where s_m is the median of the distribution $f(s)$.[12] The political–economic equilibrium will consist of platforms $(\hat{b}^m, t^1)$ and $(\hat{b}^m, t^2)$, where $\hat{b}^m$ is the optimal policy for the median voter, whose preferences are given by equation 5. In what follows, I shall study the values of t^1 and t^2 that the parties will propose in equilibrium. And to do this, I must calculate the median voter's favorite policy as a function of t^1 and t^2.

To this end, I begin by calculating the functions $L(s, b)$ and $G(t, b)$. Differentiating the expression in statement 1 of definition 2 with respect to L gives $L(s, b) = (1 - b)s/d$. (It is noteworthy that because of the quasi-linear utility function, L does not depend on (t^1, t^2)—an immense simplification, which allows us to solve for the functions $L(s, b)$ and $G(t, b)$ explicitly.) Plugging the expression for L into statement 2 of definition 3 gives

$$G(t, b) = \frac{tb(1-b)}{d}\,\mu_2(f),$$

where

$$\mu_2(f) \equiv \int_S s^2 f(s)\,ds$$

is the second moment of f. Plugging the expressions for $G(t, b)$ and $L(s, b)$ into equation 5 gives

$$v(b, s; t^1, t^2) = \frac{(1-b)^2 s^2}{2d} + \frac{b(1-b)}{d}\,\mu_2(f)\mu_1(\varphi^{ex}(t, s)), \tag{6}$$

where

$$\mu_1(\varphi^{ex}(t, s)) \equiv \int_T t\varphi^{ex}(t; s)\,dt$$

is the mean of $\varphi^{ex}(\cdot, s)$.

Next I shall make a new assumption:

ASSUMPTION 7. $s_m^2 < 2\mu_2(f)\mu_1(\varphi^{ex}(t; s_m))$.

If the mean of $\varphi^{ex}(\cdot; s_m)$ is not too much below unity, then assumption 7 is a reasonable assumption. In any case, it is assumed because it simplifies the analysis by guaranteeing that $v(b, s_m; t^1, t^2)$ is a concave function of b, whose maximum can therefore be calculated by setting the derivative of equation 6 with respect to b equal to zero:

$$-(1-b)s_m^2 + \mu_2(f)\mu_1(\varphi^{ex}(s_m))(1-2b) = 0.$$

Thus, the optimal policy for the median voter with beliefs φ^{ex} is

$$\hat{b}_m(t^1, t^2) = \max\left(0, \frac{s_m^2 - \mu_2(f)\mu_1(\varphi^{ex}(s_m))}{s_m^2 - 2\mu_2(f)\mu_1(\varphi^{ex}(s_m))}\right). \tag{7}$$

We might make a stronger version of assumption 7:

ASSUMPTION 8. $s_m^2 < \mu_2(f)\mu_1(\varphi^{ex}(t; s_m)$.

Then equation 7 implies

$$\hat{b}_m(t^1, t^2) = \frac{s_m^2 - \mu_2\mu_1}{s_m^2 - 2\mu_2\mu_1}. \tag{8}$$

Let us review the situation. I have, thus far, fixed the announcements (t^1, t^2) and calculated the median voter's ideal tax policy, $\hat{b}_m$, as a function of (t^1, t^2), given by equation 7 or 8. Now think of what (t^1, t^2) must look like at political–economic equilibrium. First, note that, at the *true* theory of the economy t_0, which, we suppose, is known by both parties, the utility of a voter s is given by

$$v(b, s; t_0) = \frac{(1-b)^2 s^2}{2d} + \frac{b(1-b)}{d}\,\mu_2(f)t_0.$$

(This is derived in the same way that I derived equation 6.) The corresponding ideal policy of voter s_i (and therefore party i) is given by

$$\hat{b}_i(t_0) = \max\left(0, \frac{s_i^2 - t_0\mu_2}{s_i^2 - 2t_0\mu_2}\right). \tag{9}$$

I shall call the corresponding value $\hat{b}_s(t_0)$, for a voter s, the *true optimal tax rate* of voter s—true because t_0 is taken to be the true theory of the economy.

It is easy to verify from equation 9 that the true optimal tax rate for voter s is decreasing in s. Therefore, the left party, which represents a constituent with small s, would like an equilibrium where $\hat{b}_m$, of equation 7 or 8, is large, and the right party would like $\hat{b}_m$ to be small. In equation 8, the parties can influence only the term $\mu_1(\varphi^{ex}(t; s_m))$ by their proposals (t^1, t^2). Differentiating $\hat{b}_m$ with respect to μ_1 gives

$$\text{sign}\,\frac{\partial \hat{b}_m}{\partial \mu_1} = \text{sign}\,\mu_2 s_m^2 > 0,$$

and so the left party wishes to render $\mu_1(\varphi^{ex})$ *large* and the right party wishes to render $\mu_1(\varphi^{ex})$ *small*. Now invoke assumption 1, which implies that the right party will propose a low value for t and the left party will propose a high value for t; for that is how they so influence the value of $\mu_1(\varphi^{ex})$.

More precisely, we can say the following. Recall that $\underline{t}$ and $\bar{t}$ are the smallest and largest possible values for the efficiency of government. (Voters, that is, assign probability zero to the event that t lies outside the interval $[\underline{t}, \bar{t}]$.) The platforms $(\hat{b}_m(\underline{t}, \bar{t}), \bar{t})$ and $(\hat{b}_m(\underline{t}, \bar{t}), \underline{t})$ constitute, for this model, the unique political–economic equilibrium, as long as $\hat{b}_m(\underline{t}, \bar{t})$ is not greater than $\hat{b}_2(t_0)$ (the true interest of the left's constituent) and $\hat{b}_m(\underline{t}, \bar{t})$ is not smaller than $\hat{b}_1(t_0)$ (the true interest of the right's constituent). I have thus shown that a strategic party of the right will argue that government is very inefficient and that a strategic party of the left will argue that government is very efficient, in an electoral environment whose voters have malleable beliefs as specified by assumption 1.

PARTY COMPETITION OVER THE LABOR-SUPPLY ELASTICITY WITH RESPECT TO THE TAX RATE

I next consider a model where voter uncertainty concerns a parameter that determines the population's labor-supply elasticity with respect to the tax rate. Traditionally, right parties have argued that this elasticity is large and negative, while left parties have argued that it is small in absolute value. Our task is to study whether these "ideologies" are not simply optimal strategies in the electoral game between class parties.

I now assume that voter preferences are given by

$$u(y, G, L) = y + G - \left(\bar{a}L + \frac{d}{2}L^2\right).$$

Each voter s knows that the coefficient on the L term in his or her own utility function is $-\bar{a}$ but is uncertain as to the value of this coefficient for other voters. The voter believes the coefficient for all other voters is a number a distributed according to a probability density $\varphi(a; s)$ on an interval A including $\bar{a}$. I assume here that the government converts tax dollars into the public good without any efficiency loss; that is, if B is total tax revenue then $G = B$.

A voter of type s whose utility function has a coefficient a and who expects G to be the value of the public good will optimize labor supply at tax rate b, by computing

$$\underset{L}{\text{argmax}}\left[(1-b)sL + G - \left(aL + \frac{d}{2}L^2\right)\right],$$

which gives an optimal labor supply of

$$\hat{L}(s, b; a) = \frac{(1-b)s - a}{d}. \qquad \textbf{(10)}$$

To simplify the argument, I assume that for the relevant values of b and for all $s \in S$ and $a \in A$, $(1 - b)s > a$ so that everyone's labor supply is positive. From this, it follows that the elasticity of labor supply with respect to b is given by

$$\frac{\partial \hat{L}(s, b; a)}{\partial b} \cdot \frac{b}{\hat{L}} = \frac{-bs}{(1-b)s - a}. \qquad \textbf{(11)}$$

Thus, labor-supply elasticity is indeed a function of a; labor-supply elasticity is high if a is large and low if a is small.

In this model, the theory of the economy concerns the value of the parameter a. Parties compete by proposing platforms (b^i, a^i), where b^i is a tax rate and a^i is the proposed value of the parameter a. If a party wishes to argue that labor-supply elasticities are large (in absolute value), it proposes a large value of a. (See equation 11 and recall that I have assumed that the denominator in that expression is always positive for relevant b.)

We can now state the definition of *economic equilibrium*.

Definition 4. *An* economic equilibrium *at a tax rate* b *when voters' beliefs are given by densities* $\varphi^{ex}(a; s)$ *is a function* $L(\cdot, b) : S \to \Re_+$ *and a function* $G(\cdot, b) : A \to \Re_+$ *such that*

1. $$L(s, b) = \underset{L}{\mathit{argmax}} \int_A ((1-b)sL + G(a, b) - (\overline{aL} + \frac{d}{2}L^2))\varphi^{ex}(a; s)\, da \text{ for all } s \in S.$$

2. $$G(a, b) = b\int_S s(\frac{(1-b)s - a}{d})f(s)\, ds,$$ *for all* $a \in A$.

$G(a, b)$ is what voter s thinks the value of the public good will be if all other voters supply labor according to equation 10 and if a is the true theory of the economy. The first expression here states that the actual labor supply of each voter is computed by maximizing the voter's expected utility. Thus, as I discussed after definition 3, the equilibrium of definition 4 is a rational expectations equilibrium, and when it attains, no voter has any reason to change posteriori beliefs φ^{ex} about how the economy works.

We can compute that

$$G(a, b) = \frac{b(1-b)}{d}\mu_2(f) - \frac{ab}{d}\mu_1(f),$$

where $\mu_i(f)$ is the ith moment of f, for $i = 1, 2$. It follows that voter s's expectation as to the value of the public good is

$$\int_A G(a, b)\varphi^{ex}(a; s)\, da = \frac{b(1-b)}{d}\mu_2(f) - \frac{b}{d}\mu_1(f)\mu_1(\varphi^{exx}(a; s)).$$

From this, it follows that the *expected utility of voter s* at the equilibrium with tax rate b is

$$v(b, s; a_1, a_2) = (1-b)s\left(\frac{(1-b)s - \bar{a}}{d}\right) - \left[\bar{a}\left(\frac{(1-b)s - \bar{a}}{d}\right) + \frac{d}{2}\left(\frac{(1-b)s - \bar{a}}{d}\right)^2\right] + \frac{b(1-b)}{d}\mu_2(f) - \frac{b}{d}\mu_1(f)\mu_1(\varphi^{ex}(a; s)), \qquad \textbf{(12)}$$

where, as in equations 5 and 6, I include the parties' proposals (a^1, a^2) as arguments of v, for they determine the posteriori densities $\varphi^{\text{ex}}(a; s)$.

I next make a further assumption:

ASSUMPTION 9. $s_m^2 < 2\mu_2(f)$.

This is a reasonable assumption, from which it follows that v is strictly concave in b (the coefficient on b^2 is negative) and hence the optimal policy for the median voter is got by setting $\partial v/\partial b$, which we can calculate from equation 12, equal to zero. In so doing, we get an expression for the median voter's favorite policy of

$$\hat{b}_m(a^1, a^2) = \frac{\mu_1(\varphi^{\text{ex}})\mu_1(f)}{s_m^2 - 2\mu_2(f)} + \text{another term not involving } \varphi^{\text{ex}}. \quad \textbf{(13)}$$

(Here, φ^{ex} means $\varphi^{\text{ex}}(a; s_m)$.) To be precise, the median voter's favorite policy is the maximum of $\hat{b}_m(a_1, a_2)$ and zero.

In equation 13, the parties can influence only φ^{ex} by their announcements of theories of the economy. This means they can influence only the first term on the right-hand side of equation 13. From assumption 9, the first term on the right-hand side of the expression for $\hat{b}_m(a_1, a_2)$ is negative, and so a *low* value of $\mu_1(\varphi^{\text{ex}})$ is associated with a *high* value for $\hat{b}_m$. Recalling the reasoning of the efficiency-of-government section, it follows that the left party, which wants the value of $\hat{b}_m$ to be high, therefore wants $\mu_1(\varphi^{\text{ex}})$ to be low, while the right party wants $\mu_1(\varphi^{\text{ex}})$ to be high. Accordingly, by assumption 1, the right party announces a high value for a, and left party, a low value for a.

But this is equivalent to saying that if assumption 1 holds, the right party will argue that the elasticity of labor supply with respect to the tax rate is high in absolute value, while the left party will argue that the elasticity is low. I have again derived apparently "ideological" views as the outcome of optimal strategic play by class-oriented parties.

CONCLUSION

I have argued that parties compete for voters not only with policy proposals but by proposing to voters different theories of how the economy works. If the parties themselves are certain about all the parameters of the political-economic environment, then there is a median voter result: in equilibrium, parties propose the same policies but different theories of the economy.

In two examples, I have studied the nature of the theories that parties put forth in equilibrium.[13] We have seen that certain views about how the economy works that are conventionally viewed as part of the conservative and liberal *Weltanschauungen* (e.g., that government is inefficient or efficient) will in fact be put forth by partisan parties (ones entirely dedicated to pursuing the interests of given constituents) as aspects of their election strategies, when, indeed, the parties have no such *Weltanschauungen*. Thus, we cannot reject the hypothesis that conservative-versus-liberal political competition involves no principled disagreements about how the economy works but is, simply, class politics, in the sense that conservative and liberal parties are tailoring their apparent "views" about how the economy works to maximize the expected utility of their different constituents in the electoral equilibrium.

APPENDIX: PROOF OF THEOREM 1

First, it is clear that $(\hat{b}^m, \hat{b}^m)$ is an equilibrium: this follows from equations 2 and 3 and lemma 1.

Second, suppose (b_*, b^*) were another equilibrium. Suppose b_* and b^* are both less than $\hat{b}^m$; then party s_1 does better by proposing $\hat{b}^m$. In like manner, b_* and b^* cannot both lie above $\hat{b}^m$. Therefore $b^* \leq \hat{b}^m \leq b_*$.

Third, I shall show that $b^* < \hat{b}^m < b_*$ is impossible. For suppose this inequality holds and b^* wins: this cannot be, for party s_1 can do better by proposing $\hat{b}^m$. Similarly, b_* cannot win; so b^* and b_* must tie.

Fourth, since some voters prefer b^* to b_* and some, vice versa, there must be an s^* such that $\bar{v}(b^*, s^*) = \bar{v}(b_*, s^*)$. By the single crossing property, s^* is unique, and it follows that $s^* = s^m$. For suppose not and that, say, $s^* < s^m$. Then, by the single crossing property, either $\bar{v}(b^*, s) > \bar{v}(b_*, s)$ for all $s > s^*$ or $\bar{v}(b^*, s) < \bar{v}(b_*, s)$ for all $s > s^*$. In either case, one of the policies is a majority winner, contradicting the fact that b^* and b_* tie.

Fifth, I show the following inequalities hold:

$$v(b^*, s_2, t_0) > v(b_*, s_2, t_0) \quad \textbf{(A-1)}$$

$$v(b_*, s_1, t_0) > v(b^*, s_1, t_0). \quad \textbf{(A-2)}$$

Suppose, to the contrary, that $v(b^*, s_2, t_0) \leq v(b_*, s_2, t_0)$. Since $b_* > \hat{b}^m > \underline{b}$, we have, by single-peakedness, $v(\hat{b}^m, s_2, t_0) > v(b_*, s_2, t_0)$. It follows from the last two inequalities that $v(b^*, s_2, t_0) < v(\hat{b}^m, s_2, t_0)$. Thus $\hat{b}^m$ is a better reply to b_* than b^* (for party s_2), a contradiction. Thus, inequalities A-1 and A-2 hold.

Sixth, since, by step 4, s^m is indifferent between b^* and b_*, it follows that for small ε, the policy $b^* + \varepsilon$ is preferred to b_* by s^m. By step 5, inequality A-1, for small ε, s_2 also prefers $b^* + \varepsilon$ to b_*. Thus, by the single crossing property, $b^* + \varepsilon$ is preferred to b_* for all $s \geq s^m$. But for small δ, $s = s^m - \delta$ also prefers $b^* + \varepsilon$ to b_* (by assumption 3), and so a majority coalition prefers $b^* + \varepsilon$ to b_*. Thus, $b^* + \varepsilon$ defeats b_*, and it follows that $b^* + \varepsilon$ is a better response to b_*, for the s_2 party, than b^*. This contradiction establishes the impossibility of the double inequality $b^* < \hat{b}^m < b_*$.

Seventh, the boundary cases (e.g., $b^* \leq \hat{b}^m < b^*$) are likewise shown to be impossible, and we conclude $b^* = b_* = \hat{b}_m$.

For completeness, I note that the Theorem 1 can be expanded to include the other possible cases, namely,

CASE A. *If* $b < \hat{b}^m$, *then, for any* $b^* \in [\underline{b}, \hat{b}^m]$, (b^*, b^*) *is a partial electoral equilibrium at* $(\overline{t_1}, \overline{t_2})$. *These equilibria are Pareto ranked, with* $(\underline{b}, \underline{b})$ *being the Pareto dominating one. All other equilibria are equivalent to one of these.*[14]

CASE B. *If* $\bar{b} > \hat{b}^m$, *then, for any* $b^* \in [\hat{b}^m, \bar{b}]$, (b^*, b^*) *is a partial electoral equilibrium at* $(\overline{t_1}, \overline{t_2})$. *A similar statement to that in part A holds.*

The proofs of these statements are available on request.

Notes

1. Austen-Smith (1990) proposes a model of legislative debate with the same basic structure: preferences of legislators are fixed, but debate can affect beliefs about the consequences of policies.

2. See Kinder and Mebane 1983 for a careful discussion of the process by which voters form theories of the economy. This is a complex topic, and I shall here make only some highly stylized assumptions about it.

3. That is, they are uncertain about the rate at which workers will reduce their labor supply as the tax rate on income increases.

4. In this model, no cost is imposed on a party for announcing a theory of the economy that is false. Ordinarily, one could justify imposing such a cost if voters could learn what the true theory of the economy is. Thus, my implicit assumption is that voters never learn much about the true theory of the economy. This may be because voters must depend on expert opinion, and there always exist experts with a wide range of opinions about how the economy works. Indeed, this does not seem to be an unreasonable assumption, given our own experience.

5. More realistically, one might wish to say that parties maximize some weighted average of constituent interests and candidate interests. I here consider the polar case of parties as perfect agents of constituents for the sake of simplicity.

6. I think this case the most interesting, because even here, we shall see that parties propose very different theories of the economy to voters. If candidates' beliefs about the true theory differ, then it is not surprising that they propose different theories.

7. Note that the function Ψ has s has one of its arguments; therefore, each voter can be influenced differently by the party announcements. In particular, some voters can ignore the theory of the economy announced by the Left party, also.

8. It is nested, in the sense that I have assumed the announcements of parties about the theory of the economy are fixed, whereas in the full model they are strategies.

9. *Strictly quasi-concave* is sufficient here. This assumption's role is just to guarantee single-peakedness of preferences.

10. The theorem, as stated, studies only one possible case, in which $\bar{b} \leq \hat{b}^m \leq \underline{b}$ holds. The other possible cases are stated at the end of the Appendix.

11. Actually, Austen-Smith is concerned with the inference process of legislators, not voters generally. It is arguable, I think, that legislators, who are professionals, evaluate information from biased politicians more rationally than does the general voter.

12. I do not here propose an endogenous theory of party formation.

13. I should emphasize that I have proved no general theorem in the government-efficiency and labor-supply-elasticity sections but have established the results for the postulated quasi-linear utility functions. The analysis becomes complex for utility functions without this property, as one ceases to be able to compute the economic equilibrium of the model in closed form. Were I to pursue such a generalization, I would begin with simulation. I can make no general claim, as a result of the present analysis, that parties representing poor voters will always propose that the government is efficient, and so on.

14. I say that two equilibria are *equivalent* if they give rise to the same observed policy, or lottery of policies.

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American Political Science Review Vol. 88, No. 2 June 1994

ECONOMIC SECURITY AND VALUE CHANGE

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Confirming Inglehart's prediction (1971) of an intergenerational shift toward postmaterialist values, a time series analysis controlling for the joint effects of inflation and unemployment demonstrates that there is a statistically significant trend toward postmaterialism in all eight West European countries for which data are available over the past two decades. Evidence from the 1981–83 and 1990–91 World Values Surveys indicates that this value shift occurs in any society that has experienced sufficient economic growth in recent decades so that the preadult experiences of younger birth cohorts were significantly more secure than those of older cohorts. Large intergenerational differences tend to be found in societies that have experienced rapid growth in gross national product per capita, and are negligible in societies that have had little or no growth. Accordingly, postmaterialism increased in 18 of the 20 societies on five continents for which we have comparable data over the past decade.

Over two decades ago, one of the authors proposed a theory of value change implying that value priorities in advanced industrial society will tend to shift away from *materialist* concerns about economic and physical security toward greater emphasis on freedom, self-expression, and the quality of life, or *postmaterialist* values (Inglehart 1971).[1] Arguing that differences between the formative socialization of young Europeans and their elders were leading younger birth cohorts to give relatively high priority to freedom and self-expression, he suggested that future intergenerational population replacement would bring about a shift toward new value priorities.

If this shift is taking place, it may have far-reaching implications. The growth of postmaterialist values appears to have contributed to a decline of social class voting (Dalton 1991; Dalton, Flanagan, and Beck 1984; Inglehart 1977, 1990; Lipset 1981) and to the rise of new social movements (Ganzeboom and Flap 1989; Inglehart 1990; Klandermans 1990; Kriesi 1989; Offe 1990; Rohrschneider 1990), particularly environmentalist movements and parties (Bennulf and Holmberg 1990; Betz 1990; Hoffmann-Martinot 1991; Müller-Rommel 1990; Rohrschneider 1993). Changing value priorities may be reshaping the nature of political cleavages and the political meaning of *left* and *right*, giving rise to a *new politics* axis. This new axis cuts across the traditional left–right dimension, characterized by radical reform parties and movements at one pole and right authoritarian parties and movements like the Christian Coalition, the National Front, and the Republikaner at the other (Huber 1989; Inglehart 1977; 1990; Kitschelt 1994; Kitschelt and Hellemans 1990; Lafferty and Knutsen 1985; Minkenberg 1990). Moreover, Dalton, Flanagan, and Beck (1984) and Inglehart (1990) argue that weakening party loyalties and low voter turnout partly reflect the established parties' failure to offer meaningful choices about the new politics issues. Conversely, Crepaz (1990) has demonstrated that the presence of a Green party or some other postmaterialist type of party enhances electoral turnout. Finally, this process of value change seems conducive to democratization throughout advanced industrial society (Gibson and Duch 1994; Inglehart 1992).

Despite this evidence, Inglehart's most basic prediction—that a trend toward postmaterialist values actually does exist—remains controversial. Earlier critics (Böltken and Jagodzinski 1985; van Deth 1983) argued that differences between the basic values of the old and the young were simply due to life-cycle effects, rather than intergenerational change. Empirical support for this position has eroded in recent years, as it became evident that given birth cohorts did *not* become more materialistic as they aged.

Since the original thesis was published, the values battery has been used in scores of countries, and more than two decades of time series evidence are now available to test it. But even in recent years, several scholars have disputed the existence of a trend toward postmaterialist values. For example, Thomassen and van Deth examine value trends in the Netherlands between 1970 and 1986; they argue that the data demonstrate "fluctuations" in values but maintain that "no dramatic *shift* of political values has occurred" (1989, 64). "There does seem to be a decline of materialism and an increase in post-materialism since 1982," they argue, "but it is unclear at present whether that is the dawn of the expected revolution or just a temporary fluctuation" (ibid.). Shively acknowledges that there has been value change among the West European public but argues that Inglehart's claim that this change is generationally driven is "fundamentally flawed" (1991, 237). Clarke and Dutt (1991) have advanced the farthest-reaching objections to Inglehart's thesis. They argue that there has been no significant trend toward postmaterialism in half of the countries he studied and that even where there is such a trend, this movement is driven by rising levels of unemployment. If high levels of unemployment contribute to postmaterialism, they argue, then Inglehart's measure and/or theory is flawed, since it claims that security is conductive to postmaterialism, whereas they find that economic *adversity* promotes postmaterialist values.

We shall demonstrate that there has been a clear trend toward postmaterialism largely resulting from intergenerational population replacement. Moreover, the growth of postmaterialism has occurred in spite of, rather than because of, rising levels of unemployment. In short, the trend toward postmaterialism results from the processes that Inglehart discussed when he first advanced his theory of value change over two decades ago.

SHORT-TERM AND LONG-TERM VALUE CHANGE

Inglehart advances two hypotheses that predict changing values. He postulates a *scarcity hypothesis*, which states that "an individual's priorities reflect one's socioeconomic environment" and a *socialization hypothesis*, which postulates that "to a large extent, one's basic values reflect the conditions that prevailed during one's preadult years" (1985, 103). "The scarcity hypothesis" Inglehart writes, "implies short-term changes, or period effects: periods of prosperity lead to increased postmaterialism, and periods of scarcity lead to materialism. The socialization hypothesis implies that long-term cohort effects *also* exist" (ibid). During the traumatic recessions of the mid-1970s and early 1980s, period effects were substantial. Inglehart (1985) concluded that between 1970 and 1982 period effects and cohort effects were both important, with period effects actually outweighing cohort effects.

Inglehart's theory of value change predicts that intergenerational population replacement will gradually lead to a long-term shift from materialist to postmaterialist values. It not only helps predict the *direction* of future change but also the *magnitude* of the change that one would expect from generational replacement, which is not the only factor involved but *is* a major component of value change (see Abramson and Inglehart 1986, 1987, 1992; Inglehart 1990).

Over short periods, the magnitude of change predicted on the basis of population turnover is always small. If one uses Inglehart's overall measure of values, namely, the percentage difference index (the percentage of postmaterialists minus the percentage of materialists), the model predicts a change of only about one point on the index per year.[2] Given the small size of these predicted effects, short-term forces (or sampling error) can easily swamp the effects of population turnover, but in the long term, its cumulative effects can be massive.

For this reason, it is important to analyze as long a times series as possible. In their analysis of the Netherlands, Thomassen and van Deth study the years between 1970 and 1986, using Inglehart's four-item measure of materialist/postmaterialist values. Clarke and Dutt analyze only the data collected from 1976 to 1986, although they could have also analyzed the surveys conducted in 1970, 1971, and 1973 and possibly some of the more recent surveys, as well.[3] Like Thomassen and van Deth, Clarke and Dutt employ Inglehart's four-item value measure. Clarke and Dutt argue that there was no trend toward postmaterialism in Belgium, France, and Italy and that the trend toward postmaterialism was not statistically significant in Ireland. Unlike Thomassen and van Deth, however, Clarke and Dutt do find a significant movement toward postmaterialism in the Netherlands, as well as in West Germany, Britain, and Denmark.

We shall analyze all the available data from 1970–71 through 1992 for West Germany,[4] Britain, the Netherlands, France, Belgium, and Italy[5] and all of the surveys conducted in Denmark and Ireland between 1973 (when surveys started in these countries) and 1992. We shall assess whether or not there is a trend toward postmaterialism using the full time series for each country. We shall also analyze the 1990–91 World Values Survey and show that rising economic security contributes to postmaterialist values in societies with widely varying institutions and cultural heritages and at various levels of economic development. Finally, we will compare data from the 1981–83 World Values Surveys and the 1990–91 World Values Surveys to demonstrate that the predicted trend toward postmaterialism took place in 18 of the 20 societies that can be compared during this decade.

LONG-TERM VALUE CHANGE

Figure 1 shows the percentage of postmaterialists minus the percentage of materialists based on the four-item index for eight West European countries from the beginning of the time series through the fall of 1992, as well as for the United States.[6] It is apparent that there has been a trend toward postmaterialism in seven of these eight European societies, as well as the United States. Only the Belgian data fail to reveal a trend toward postmaterialist values.

Figure 1A presents the results for Britain and France. (Presenting the data for all nine countries at once would produce an unreadable graph.) Both countries show an overall trend toward a rising proportion of postmaterialists, but here (as elsewhere), this trend is punctuated by dips toward materialism that reflect the recessions of the mid-1970s and the early 1980s. There is also a downward fluctuation at the end of the series, reflecting the current global recession. This effect is noticeably more pronounced for Britain (where the latest recession has been more severe) than in France. For Britain, the shift toward postmaterialism reached its peak in 1989, when the inflation rate was 7.8% annually and unemployment was 7.0%. By early

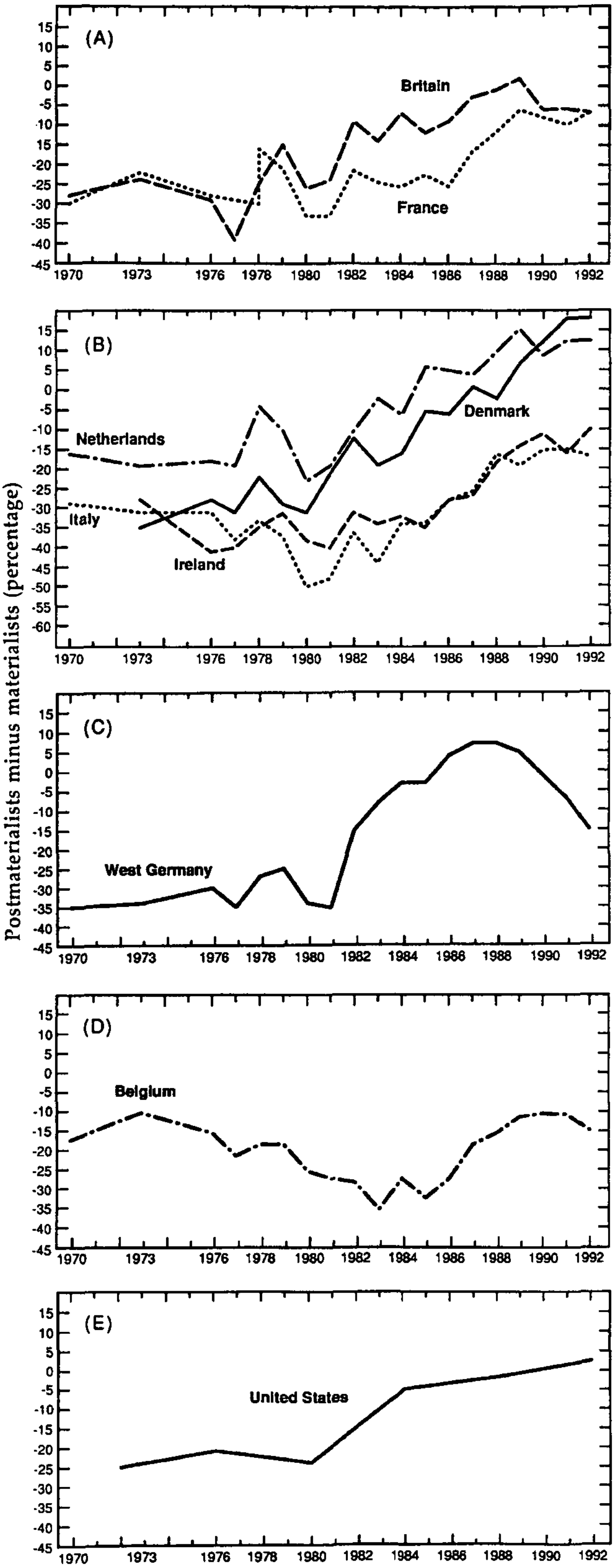
1992, these figures had risen to 10.4% and 10.3%, respectively. In France, on the other hand, unemployment rose only slightly (from 9.4% in 1989 to 9.9% in 1992), while inflation remained at 3.7% in both years. Even though both inflation and unemployment were higher at the end of the time series than at the beginning, there was a substantial overall trend toward postmaterialist values. In Britain, for example, at the start of the series in 1970–71, materialists (36%) were five times as numerous as postmaterialists (7%), producing an index of −29 (with the remainder having mixed values). By 1992, 24% were materialists and 16% were postmaterialists, producing an index of −8: postmaterialists were now two-thirds as numerous as materialists. In France, the figures changed from 41% materialists and 11% postmaterialists in 1970–71, to 27% materialists and 19% postmaterialists in 1992.

Figure 1B presents the results from four more countries, two of which had even stronger shifts from materialist to postmaterialist values than those found in Britain or France. The Netherlands, Denmark, Ireland, and Italy all show downward fluctuations linked with the global recessions of the mid-1970s and early 1980s, plus a leveling-off or downward movement linked with the latest recession at the end of the time series—but all four show clear trends from materialist toward postmaterialist values.

Although the West German data (see Figure 1C) show the same overall trend toward postmaterialism, they manifest a much larger downward shift between 1988 and 1992. After the wall came down in 1989, West Germany absorbed East Germany, a society with a political system that had collapsed and an economy in free fall. East Germany's problems became West Germany's problems, with huge sums being transferred from the west to help reconstruct Eastern Germany. Inflation rose from 1.3% in 1988 to 4.0% in 1992 and though unemployment remains fairly low in the West, it has risen to appalling levels in the East (with some estimates placing it at 30%). In addition, following the collapse of the socialist regimes in Eastern Europe, hundreds of thousands of political and economic refugees fled to Germany, giving rise to a pervasive sense of insecurity and severe xenophobia, linked with attacks on foreigners. Despite the severity of recent conditions, West Germany nevertheless shows a significant long-term shift from materialist to postmaterialist values over the past two decades. In 1970 (with much lower rates of inflation and unemployment than in 1992), 45% of the West Germans were materialists and only 10% were postmaterialists, producing a percentage difference index of −35. In 1992, the proportion of materialists had fallen to 31%, and that of postmaterialists had risen to 14%, for an index of −16. This overall upward movement of the index, in the face of strong, short-term, downward pressures, resulted largely from intergenerational population replacement processes in which younger, relatively postmaterialist birth-cohorts replaced older, relatively materialist cohorts.

FIGURE 1

Percentage of Postmaterialists Minus Percentage of Materialists in Eight West European Societies and the United States: 1970 through 1992.



Belgium is the only remaining country for which we have detailed survey data with numerous regular measurements covering a long span of years (see Fig. 1D). The Belgian time series shows no clear trend toward postmaterialist values. At the end of the series in 1992, postmaterialism among the Belgian public was only marginally higher than it was in the 1970–71 baseline surveys.

Finally, though no detailed time series is available for the United States like that produced by the Euro-Barometer's semiannual surveys, the four-item materialist/postmaterialist values battery has been included in each of the National Election Study (NES) presidential election surveys from 1972 through 1992. Figure 1E shows the results: again, we find the predicted long-term upward trend, with the ratio rising from 35% materialists and 10% postmaterialists in 1972, to 16% materialist and 19% postmaterialists in 1992.

Inglehart's thesis predicts only modest gains of about one point per year in postmaterialism, since he argues that the trend is driven by generational replacement. If generational replacement were the only factor influencing value change, the trend toward postmaterialism would be similar in all these societies, since the speed of generational replacement varies only slightly from country to country (see Abramson and Inglehart 1992). As we shall see, economic factors do influence short-term value change. Before examining the dynamics of long-term and short-term value change, it is important to illustrate that the trend toward postmaterialism is robust in the face of alternative values measures.

Inglehart has also developed a broader indicator of values based upon a choice among 12 national goals.[7] As Inglehart has demonstrated (1977, 1990), this 12-choice measure is more reliable and valid than the widely employed measure based upon a choice of four goals and less sensitive to the effects of inflation. Unfortunately, the more complex index is also more time-consuming and expensive to administer and, as a consequence, has been used in relatively few national surveys. However, we can examine levels of materialism/postmaterialism using this 12-choice measure with surveys conducted in 1973 and 1990. The results from these eight European societies are presented in Table 1. These data reveal a shift toward postmaterialism in all eight countries, although the shift in Belgium is relatively modest. Across the eight countries for which data covering this 17-year period are available, the percentage scoring "high" on the materialist/postmaterialist index rose from 41% in 1973 to 54% in 1990. As Inglehart (1990) has shown, young Europeans are much more likely to score as postmaterialists on this measure than older Europeans are. Though there are not enough observations for a detailed analysis of the dynamics of value change, the trend toward postmaterialism also emerges when the more reliable 12-item measure is used.

TABLE 1

The Shift toward Postmaterialist Values: Results from the 12-Item Index in 1973 versus 1990

	PERCENTAGE SCORING "HIGH" ON 12-ITEM INDEX[a]		
COUNTRY	1973	1990	SHIFT
West Germany	25	53	+28
Italy	34	52	+18
Denmark	35	51	+16
Netherlands	53	69	+16
Britain	38	50	+12
Ireland	38	46	+8
France	50	58	+8
Belgium	51	55	+4
Mean	41	54	+13

Source: 1973 European Community Survey and 1990 World Values Survey.

[a] This index is constructed by summing the scores from choosing the five postmaterialist goals, when a score of +2 is given if the given item is ranked highest in its group of four items, and a score of +1 is given if it is ranked second highest in its group. Scores on the resulting index range from 0 to 8; a "high" score is one of 4 or higher.

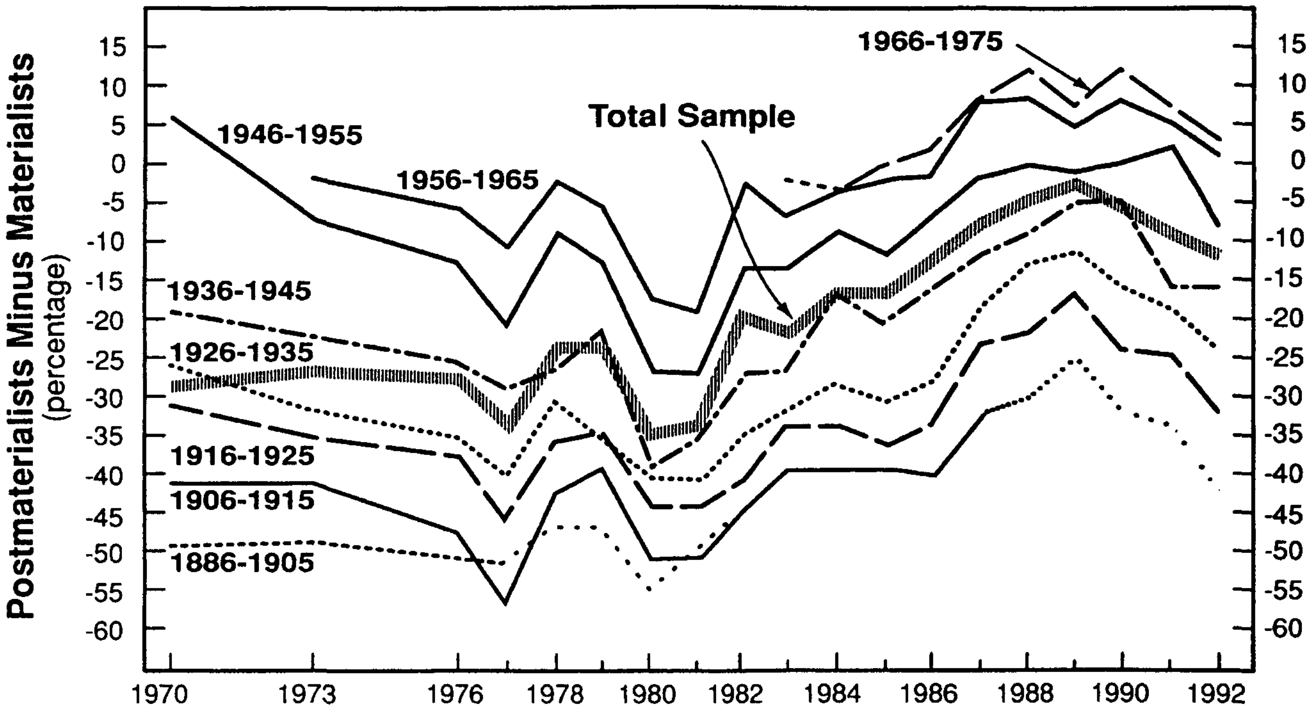
WHAT CAUSES LONG-TERM CHANGE?

Inglehart's thesis maintains that the trend toward postmaterialism results mainly from generational replacement. We have earlier demonstrated that replacement is a driving force behind value change (Abramson and Inglehart 1986, 1987, 1992), but we can provide additional evidence. Figure 2 presents a cohort analysis (using the four-item measure) among eight birth cohorts tracked from 1970 to 1992 using a combined weighted sample of Germany, Britain, the Netherlands, France, Belgium, and Italy.[8] Vertical comparisons between birth cohorts in each year reveal that younger cohorts consistently have higher levels of postmaterialism than their elders. These relationships are not just a function of the higher educational levels of younger Europeans, although the very low educational levels of older Europeans contributes to their levels of postmaterialism.[9] Following given cohorts from left to right across the figure tracks the value fluctuations of each cohort as it ages. For example, when it was first sampled, the cohort born from 1946 to 1955 was between the ages of 15 and 24. By 1992, this cohort was between the ages of 37 and 46.

There are obvious short-term fluctuations for every birth cohort. But by following given cohorts from left to right across the figure, it becomes clear that there is no overall tendency for birth cohorts to become more materialist as they age. That cohorts do not become more materialist as they age undermines a life-cycle interpretation for the relationship between age and materialism. But as Shively (1991) correctly argues, such evidence does not conclusively disprove a life-cycle interpretation. As Glenn (1977) has shown, one

FIGURE 2

Cohort Analysis, 1970–92.



Source: Based on combined weighted sample of European Community Surveys carried out in West Germany, France, Britain, Italy, the Netherlands, and Belgium, in given years, using the four-item materialist/postmaterialist values index (N = 222,699).
Note: % postmaterialists – % materialists in eight birth cohorts and for total sample in six West European societies.

can never definitely choose between a life-cycle and a generational explanation on statistical grounds alone, for in any cohort matrix the age of a cohort is a perfect function of its date of birth and the time that the survey is conducted. Even though Figure 2 demonstrates that these cohorts did not become more materialist as they aged, one could always argue that some unmeasured period effect conducive to postmaterialism suppressed true life-cycle forces that would otherwise make the cohorts more materialist as they aged. On the other hand, as Shively also notes, analysts *can* distinguish period effects from cohort effects and life-cycle effects if they have side information that enables them to identify any one of the three. We have two important pieces of side information: (1) we know that major economic recessions affected all six of these countries in the mid-1970s, the early 1980s, and again in the early 1990s; and (2) theoretically, these economic conditions would be expected to produce a downward shift toward materialism, since they lead to a diminished sense of economic security. This interpretation was proposed by Inglehart (1981, 1985) to account for the downward swings in postmaterialism in the mid-1970s and the early 1980s. We now have an additional test of this interpretation: another recession occurred in the early 1990s, and we find another downward swing in the proportion of postmaterialists. If we accept the interpretation that these fluctuations in the values indicator are, indeed, due to these recessions, then we have identified the major period effects. The remaining pattern over time is essentially horizontal for each cohort, which implies that there are no important life-cycle effects and that the enduring intercohort differences reflect generational change. We find this interpretation quite plausible and think most other observers will also: it fits the original theory neatly, with each successive recession producing a shift toward materialism, followed by a resurgence of postmaterialist values when economic recovery takes place.

Even though we cannot definitively rule out the possibility that these cohorts might eventually become more materialist as they age, the overall increase in postmaterialist values that we observe here results largely from generational replacement. This can be seen by comparing the overall trend line in Figure 2 with the trend lines for the eight cohorts.

Despite substantial short-term fluctuation, given cohorts change very little from 1970–71 to 1992. But during these years the overall percentage difference index rises from −29 to −10, a 19-point gain. None of the cohorts tracked over these years registered a gain that is even close to this magnitude (they show virtually no gain at all). The rise in the overall percentage difference index occurs because during these 22 years, the older, more materialist cohorts were gradually dying and being replaced by younger, more postmaterialist cohorts. These results make clear that generational replacement is a major force driving postmaterialism upward. The net rise in postmaterialism clearly is *not* due to changes in inflation rates, even though the short-term fluctuations within any given cohort do track the inflation rate very closely (as Figure 3 will demonstrate), for the overall inflation rate at the end of this period was *higher* than at the start (6.5% in 1992, 5.2% in 1970), but the proportion of postmaterialists nevertheless reveals a long-term increase. In 1970–71, the six-nation sample examined in Figure 2 contained 40% materialists and 11% postmaterialists, with materialists outweighing postmaterialists by nearly 4:1. By Spring 1992, the figures had shifted to 27% materialists and 16% postmaterialists, for a ratio of less than 2:1. Overall gains in postmaterialism result mainly from the gradual exit of the older cohorts through death and their replacement by cohorts with higher levels of postmaterialism. Although generational replacement is gradual, over the course of two decades, it becomes substantial. According to census statistics, by the end of 1992, 40% of the adults in these six societies were too young to have been sampled in 1970. In other words, between 1970 and 1992, two-fifths of the adult population had been replaced. Obviously, a theory of value change based upon intergenerational population replacement is compatible with substantial short-term variation. But just what causes that short-term variation?

WHAT CAUSES THE PERIOD EFFECTS?

Although there definitively is a trend toward postmaterialism in seven of these eight European societies, none of them shows a simple linear progression toward postmaterialist values (as Figure 1 demonstrates). Clearly, short-term period effects are present. Inglehart's theory predicts that economic and physical insecurity will tend to depress the proportion of postmaterialists and that relatively secure conditions will be conducive to increases in postmaterialism. The four-choice value battery employed to measure materialist/postmaterialist values includes one choice that makes the measure sensitive to changing rates of inflation, since one of the four proposed national goals is "fighting rising prices."[10] Indeed, Inglehart has demonstrated that much of the short-term variation in values results from changing levels of inflation. Using just two variables—(1) cohort effects and (2) period effects (as measured by change in the consumer price index)—he explained 87% of the cohort-level variation in value priorities in a combined sample of six Western European publics between 1970 and 1982 (Inglehart 1985). We can now examine the relationship between cohort change and inflation rates over a much longer period.

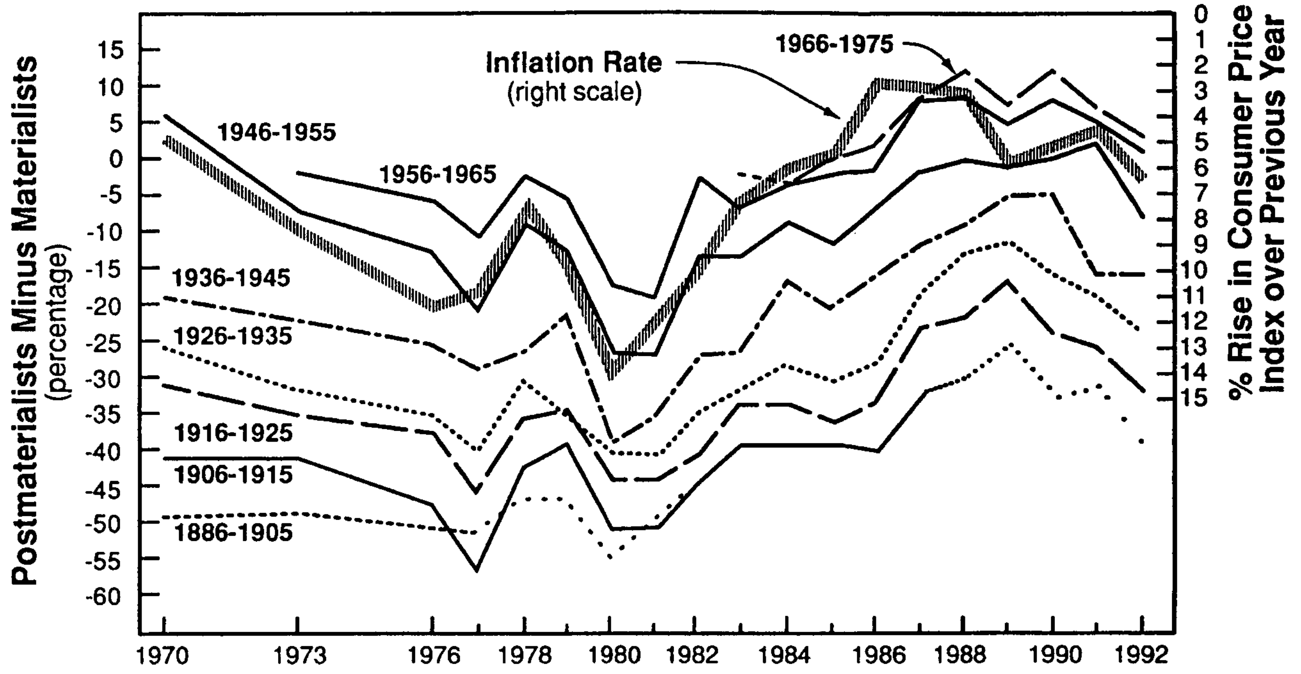
Figure 3 presents the results of a cohort analysis of the pooled results from the six countries that have been surveyed since 1970, but we now superimpose the current rate of inflation.[11] Since Inglehart's theory predicts that postmaterialist values will rise when inflation falls, the inflation index runs from low rates at the top of the graph to high rates toward the bottom. This makes it easy to see that (as predicted) inflation and postmaterialist values move up and down together, bearing in mind that a *downward* movement of the inflation line indicates *rising* rates of inflation.

Substantial period effects occur during these years, as even a cursory glance at Figure 3 suggests. Though the long-term tendency is for each cohort to remain about as materialist or postmaterialist as it was when it was first sampled, we find three pronounced dips in the proportion of postmaterialists; they correspond to the recessions of the mid-1970s, the early 1980s, and the early 1990s. As predicted, period effects are closely linked with the inflation level at a given time. At most time points, the percentage difference index for each cohort moves up or down with the current rate of inflation. When we control for the effects of inflation, the period effects largely disappear. But statistical controls are not needed in the present example. As it happens, by 1990, the weighted mean inflation rate for these six countries had returned to almost exactly the same level that it had in 1970. Thus, when we compare the 1970 results with the 1990 results, the main source of short-term period effects (inflation) has been controlled for by nature. With inflation thus controlled, note that the percentage difference index for these six countries rose from −29 to 1970 to −6 in 1990, a net gain of 23 points. In other words, over this 20-year period, the balance between materialists and postmaterialists shifted by 1.15 points per year—the very amount that would take place from the intergenerational population replacement process alone. In any given year, current period effects will add to, or subtract from, this amount, but this is the long-term trend. The finding that short-term change results largely from changes in inflation is clearly consistent with Inglehart's scarcity thesis. Moreover, as we shall see, in seven of the eight countries for which detailed data are available, short-term value change results mainly from changing inflation rates.

Clarke and Dutt propose an alternative source of short-term variation that is not consistent with the scarcity hypothesis. They argue that contrary to what one might expect, rising levels of unemployment are

FIGURE 3

Cohort Analysis with Inflation Rate Superimposed (Using Inverted Scale on Right), 1970–92.



Source: Based on combined weighted sample of European Community Surveys carried out in West Germany, France, Britain, Italy, the Netherlands, and Belgium, in given years, using the four-item materialist/postmaterialist values index (N = 222,699).
Note: % postmaterialists – % materialists in six West European societies.

conducive to postmaterialist values. They argue that this happens because respondents cannot choose fighting unemployment as a goal, so that when unemployment rises, they tend to choose the option of "Giving the people more say in important government decisions" to express their concern about unemployment.

The rationale for expecting such a relationship is much less straightforward than the relatively obvious linkage between high levels of inflation and the goal of fighting rising prices. But if high levels of unemployment *do* in fact contribute to postmaterialism, this relationship should be found at the individual level. Clarke and Dutt present some evidence from the 1980–81 wave of a panel study conducted in West Germany, the Netherlands, and the United States that they claim demonstrates this linkage. This survey includes a question about jobs, but it is ambiguous. Respondents are asked, "Which of the following problems do you consider most important?" Immediately after the item "Guaranteeing equal rights for men and women," the next choice is "Seeing to it that everyone who wants a job can have one." This item could tap either the respondent's *own* anxiety about becoming unemployed or concern for fair employment practices for *everyone*, since it explicitly refers to jobs for "everyone who wants one" and appears immediately after an item about "equal rights for men and women." In any event, this ambiguous item is weakly related to postmaterialist values. Among those who make it the main concern, 15% are postmaterialists on the 12-choice index; among the entire 1980–81 cross section as a whole, 12% are postmaterialists.

Although this item yields weak and ambiguous results, there is considerable evidence demonstrating the opposite relationship namely, that at the individual level, concern about unemployment is conducive to *materialist* values, rather than to *postmaterialist* values. Let us briefly review this evidence.

In developing a measure of materialist/postmaterialist values in 1970, Inglehart actually *did* experiment with an item that taps concern about unemployment. He employed a four-choice battery that included the following choices:

> I will now propose a number of concrete objectives. Among the following things, which are the two that seem most desirable to you?
>
> 1. Guarantee greater job security
> 2. Make our society more humane
> 3. Raise salaries
> 4. Guarantee the participation of workers in the management of enterprises.

This measure was employed in five countries, in addition to the now-standard four-item battery. How these four objectives relate to postmaterialism is presented in Table 2. The table demonstrates that

TABLE 2

Percentage of Postmaterialists, by Choice Among Four Concrete Objectives in Five West European Societies, 1970

OBJECTIVE CHOSEN AS MOST IMPORTANT	PERCENTAGE CLASSIFIED AS POSTMATERIALIST					
	WEST GERMANY	NETHERLANDS	FRANCE	BELGIUM	ITALY	WEIGHTED MEAN
Raise salaries	5	3	2	8	4	4
Guarantee greater job security	6	15	7	12	10	8
Overall sample	11	19	11	14	13	12
More participation by workers	12	22	12	10	26	16
Make our society more humane	26	30	22	21	22	24

Source: 1970 European Community Survey.
Note: Based on four-item values index.

respondents who emphasized job security are consistently *less* likely than the overall public to be postmaterialists (as Inglehart's theory implies).

The 1973 European Community survey provides further individual-level evidence that contradicts the thesis that concern for unemployment gives rise to postmaterialism. Respondents in eight countries were asked the following question:

> Here are some things people usually take into account in relation to their work. Which one would you personally place first? . . . and which next?
>
> 1. A good salary so that you do not have any worries about money
> 2. A safe job with no risk of closing down or unemployment
> 3. Working with people you like
> 4. Doing an important job which gives you a feeling of accomplishment.

Unlike both the 1970 question and the 1980–81 question, which could be read as tapping a sociotropic concern to provide greater job security or fair employment practices for everyone, this 1973 item explicitly refers to concerns that "you personally" would take into account. And here, the relationship between job goals and materialist/postmaterialist values is unambiguous, as we see in Table 3. The pattern is striking. Respondents who emphasize "a safe job with no risk of unemployment" are about *half* as likely to have postmaterialist values as those who choose "working with people you like" or "a job that gives you a feeling of accomplishment." In all eight countries those who choose "a safe job" are *less* likely to score as postmaterialists than the overall sample. Moreover, similar results obtain when one examines the relationship between employment goals and a measure of materialism/postmaterialism based upon a choice of twelve national goals. Far from being conducive to postmaterialist responses, a concern with unemployment is linked with *materialist* responses (as the scarcity hypothesis implies).

During certain periods, rising levels of unemployment *did* coincide with aggregate-level increases in postmaterialism. This can be seen in Figure 4, which

TABLE 3

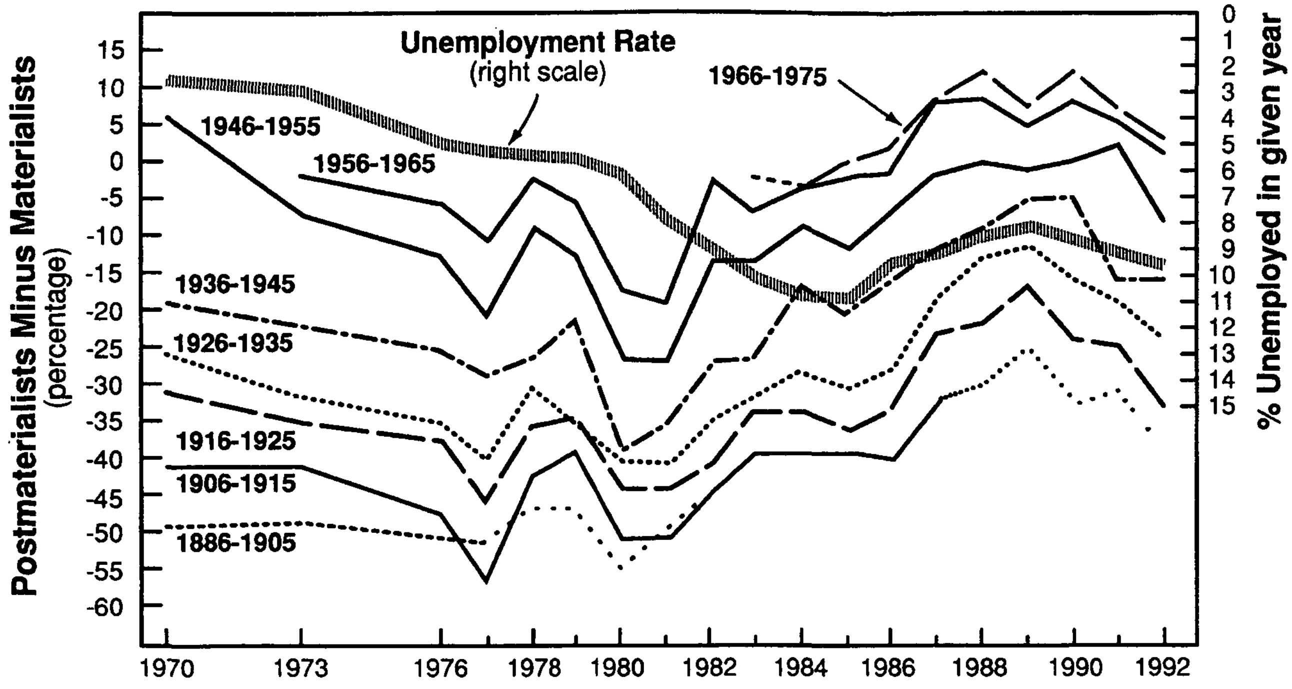
Percentage of Postmaterialists, by Occupational Goals in Eight West European Societies, 1973

GOAL CHOSEN AS MOST IMPORTANT	PERCENTAGE CLASSIFIED AS POSTMATERIALIST								
	WEST GERMANY	GREAT BRITAIN	NETHERLANDS	FRANCE	BELGIUM	ITALY	DENMARK	IRELAND	MEAN
A good salary	3	4	4	6	8	6	4	6	5
A safe job	4	6	8	9	10	4	8	4	7
Overall sample	9	8	12	13	14	9	12	8	11
Working with people you like	13	9	17	19	21	13	17	8	15
A feeling of accomplishment	21	14	18	20	21	16	18	14	18

Source: 1973 European Community Survey.
Note: Based on four-item values index.

FIGURE 4

Cohort Analysis with Unemployment Rate Superimposed (Using Inverted Scale on Right).



Source: Combined weighted representative national surveys from France, West Germany, Britain, Italy, Belgium, and the Netherlands, including European Community surveys from 1970 and 1973 and Euro-Barometer surveys 6 through 37 (over 220,000 interviews).
Note: % postmaterialist – % materialist in six West European societies.

(like Figures 2 and 3) shows percentage difference index levels among the eight birth cohorts. For this figure, however, we superimpose unemployment rates. Unemployment is indexed so that a downward movement in the graph indicates higher rates of unemployment. Bear in mind that Clarke and Dutt's analysis is limited to the period from 1976 through 1986.

The most striking feature of the relationship between unemployment and materialist/postmaterialist values is its *inconsistency* over time. The contrast with the consistent relationship between inflation and values is impressive. From 1970 to 1977 and from 1985 to 1992, the unemployment index moves in the *same* direction as the value index; that is, higher rates of unemployment are linked with movement toward materialism, not postmaterialism. But the relationship reverses itself during most of the period studied by Clarke and Dutt. Here, rising rates of unemployment *were* accompanied by value shifts toward postmaterialism, as they claim. This pattern does not hold up even throughout the entire period from 1976 through 1986. During the first year of this period, unemployment is rising, accompanied by a shift toward materialism and during the last year of this period, unemployment is falling, accompanied by a shift toward materialism, and during the last year of this period, unemployment is rising, accompanied by a shift toward materialism, and during the last year of this period, unemployment is falling, accompanied by a shift toward postmaterialist values. But the relationship reported by Clarke and Dutt does prevail from 1981 to 1985. During these years—and only during these years—a resurgence of postmaterialism was accompanied by a steep rise in unemployment. A glance back at Figure 3 reveals that this steep rise in unemployment went along with an even steeper drop in inflation, bringing a sharp rise in postmaterialist values.

A more rigorous test to determine whether there is a significant trend toward postmaterialism can be conducted using a time series regression analysis. Moreover, because statistical analyses are parsimonious, we can examine change in all eight societies, as well as the combined European sample. The results of our time series analysis are presented in Table 4. We present the results of a time series regression analysis (employing ordinary least squares) in which the materialist/postmaterialist values index is the dependent variable and in which the number of years from the baseline survey, the annual inflation rate, and the annual unemployment rate are the independent variables.

The last two columns of Table 4 present diagnostic

TABLE 4

Predicting Postmaterialist Values in Eight West European Societies, 1970/71–92

COUNTRY	B	SE B	BETA	T	(SIG. 2-TAILED)	R^2	DW	ACF (1)
West Germany								
Years since baseline	1.31	.28	.50	4.64	.0003	.89	1.36	.263
Inflation	−4.20	.91	−.45	−4.60	.0003			
Unemployment	1.60	.79	.22	2.02	.0616			
Britain								
Years since baseline	1.21	.31	.66	3.91	.0014	.78	2.01	−.047
Inflation	−.99	.36	−.40	−2.75	.0148			
Unemployment	−.20	.64	−.06	−.32	.7568			
Netherlands								
Years since baseline	1.29	.25	.63	5.18	.0001	.88	2.02	−.012
Inflation	−2.39	.56	−.50	−4.24	.0007			
Unemployment	−.58	.36	−.17	−1.62	.1261			
France								
Years since baseline	1.82	.54	1.34	3.38	.0041	.73	2.19	−.113
Inflation	−.60	.39	−.27	−1.51	.1510			
Unemployment	−2.54	1.01	−.89	−2.52	.0235			
Belgium								
Years since baseline	.79	.20	.62	4.01	.0011	.85	2.11	−.136
Inflation	.09	.43	.03	.22	.8278			
Unemployment	−2.41	.29	−1.05	−8.29	.0000			
Italy								
Years since baseline	1.07	.32	.63	3.29	.0050	.85	1.84	.066
Inflation	−1.46	.23	−.76	−6.47	.0000			
Unemployment	−2.23	.74	−.54	−3.01	.0088			
Denmark[a]								
Years since baseline	2.65	.59	.82	4.46	.0005	.94	1.63	.145
Inflation	−.96	.80	−.19	−1.20	.2491			
Unemployment	−.48	.75	−.06	−.64	.5301			
Ireland[a]								
Years since baseline	2.07	.38	1.09	5.47	.0001	.87	1.47	.116
Inflation	−.96	.26	−.57	3.78	.0020			
Unemployment	−2.00	.47	−.84	−4.25	.0008			
Six European Societies[b]								
Years since baseline	1.03	.25	.61	4.09	.0010	.89	1.84	.003
Inflation	−1.62	.32	−.53	−4.98	.0002			
Unemployment	−.40	.53	−.10	−.76	.4597			

Source: Data about values are based upon European Community Surveys. Data about inflation and unemployment are from the Statistical Office of the European Communities. For the results employing iterative Prais–Winsten estimators, see Inglehart and Abramson 1993, tbl. A1.

Note: Values are percentage difference indices using the four-item measure (ordinary least squares).

[a] Baseline survey is 1973.

[b] Combined weighted sample of West Germany, Britain, the Netherlands, France, Belgium, and Italy.

statistics (the Durbin–Watson test and a first-order autocorrelation function coefficient) on the residuals that test for the presence of autocorrelation. Both statistics reveal that there is no problem with autocorrelation,[12] and we can therefore employ these ordinary least squares results.[13]

As Table 4 shows, when we simultaneously control for the effects of inflation and unemployment, there is a significant trend toward postmaterialism in all eight countries, *including* Belgium. Of course, we know from Figure 1D that actual levels of postmaterialism increased only negligibly in Belgium. These analyses suggest that there would have been a substantial increase in postmaterialism in Belgium if unemployment had not contributed to *materialism.* Table 4 also shows that inflation has a statistically significant effect on reducing postmaterialism in every country except France, Belgium, and Denmark. Unemployment has a statistically significant impact on the percentage difference index in Belgium, as well as in France, Italy, and Ireland. But in all four countries, unemployment contributes to materialism. The positive relationship between unemployment and postmaterialism comes close to statistical significance in Germany but falls far short of significance when the minimal effects of autocorrelation are removed (see Inglehart and Abramson 1993, table A1).

The crucial finding is that when we take the effects of inflation and unemployment into account, there is a statistically significant trend toward postmaterialism in all eight societies. Moreover, this change is very much in the order of magnitude predicted by Inglehart's thesis—about a one-point gain per annum on the index. For the combined six-nation sample, there is a 1.03-point per year increase in the index; and this result is statistically significant at the .001 level. Inflation also affects values, a result that is statistically significant at the .0002 level. Unemployment is negatively related to postmaterialism, although the relationship is negligible (the significance level is .4597).

These results strongly support Inglehart's (1971) thesis that there would a trend toward postmaterialism.[14] Moreover, Inglehart was making actual ex ante predictions, not ex post predictions, in which a theory can be designed to fit the observed results. Indeed, the theory predicting the shift toward postmaterialist values was published many years before most of the data used to test it had been collected.

The argument that high rates of unemployment should be conducive to postmaterialism seemed implausible from the start. Although at certain times high rates of unemployment happened to coincide with a rise in postmaterialism, the apparent causal linkage is spurious.

UNEMPLOYMENT AND POSTMATERIALISM IN EX-SOCIALIST COUNTRIES

The evidence we have examined so far is based on only relatively modest changes in unemployment rates. Recent events in the ex-socialist world provide a stronger test of this argument. Within the past few years, these countries have gone from being socialist societies, in which unemployment was quite low, to an abrupt transition to market economies, in which unemployment has suddenly become unprecedentedly widespread. If unemployment is conducive to postmaterialist values, we should observe a sharp rise in postmaterialism in these societies since the collapse of socialism. On the other hand if, as we argue, insecurity is conducive to materialist values, we should find a dramatic shift toward materialism in the last few years.

Among the ex-socialist societies, East Germany provides a particularly good site for testing these two competing interpretations. For one thing, it has experienced a severe rise in unemployment. Since the collapse of the socialist regime in 1990, entire sectors of East German industry have proven to be noncompetitive by Western standards and have closed down completely. Unemployment has risen from an official rate near zero to traumatic levels that are partly concealed by labeling unemployment payments as retraining salaries but that some observers estimate to include as much as 30% of the work force. Moreover, we can test the proposition readily in this setting because, from reunification to the present time, Euro-Barometer surveys have been carried out, with separate samples in each of the two former regions of Germany each spring and fall.

Let us examine the results. Did the massive rise in unemployment bring a surge in postmaterialist values, as Clarke and Dutt's interpretation implies? Or did it bring a sharp decline in postmaterialism, as the value-change thesis implies? The answer is clear and unequivocal. In Fall 1990, when the German Democratic Republic was abolished, Euro-Barometer Survey 34 found that 21% of the East German public fell into the materialist category, 65% were mixed, and 14% were postmaterialists, yielding a percentage difference index of −7. While this was well below the West German level of postmaterialism, East Germany was probably the most postmaterialist society in the ex-socialist world. But with the subsequent dismantling of large parts of the East Germany economy, this index fell sharply, dropping to −9 in Spring 1991, to −19 in Fall 1991, to −26 in Spring 1992, and to −24 in Fall 1992. Within two years, the proportion of postmaterialists had been cut in half. Here again, the notion that high levels of unemployment are conducive to postmaterialism is contradicted by the empirical evidence.

ECONOMIC SECURITY AND VALUE CHANGE: NEW EVIDENCE FROM 40 SOCIETIES

The World Values Surveys carried out in 1981–83 and 1990–91 provide further evidence that economic security contributes to the rise of postmaterialist values.[15] Though they do not provide the detailed time series that is available from the Euro-Barometer surveys, they do provide a broader coverage of the developmental spectrum that has ever previously been available. The 1981–83 surveys conducted representative national samples in 22 societies; apart from a small sample in Northern Ireland (about 300 cases in both surveys), the sample sizes ranged from 927 to Iceland to 2,303 in Spain. The 1990–91 surveys provide data from representative national samples from 40 societies containing over 70% of the world's population, with sample sizes ranging from 588 in Finland to 4,147 in Spain, and it employed the 12-choice value battery. (We give the actual number of respondents in each society that received a score on the 12-choice measure of materialism/postmaterialism in Inglehart and Abramson 1993, tbl. A2). These surveys cover an unprecedentedly broad range of the economic and political spectrum, with data from such low-income nations as China, India, and Nigeria, as well as from ex-socialist countries in Eastern Europe and from advanced industrial democracies. Both World Values Surveys measured materialist/postmaterialist values in every inhabited continent except Australia.

The value change thesis implies that the shift from materialist to postmaterialist priorities is potentially a universal process: it should occur in any country that moves from conditions of economic insecurity to relative security (though during a transitional period, older generations will continue to reflect the conditions that characterized their preadult experiences). This has clear implications concerning the linkage between economic development and value change: though a high level of per capita income does not *necessarily* produce a sense of security, it seems likely that people living in rich countries will experience a stronger sense of economic security than those in poor nations. A high gross national product per capita is at least a rough indicator of a country's level of economic security. In short, economic development should be conducive to a shift from materialist to postmaterialist values.

Though this implication is straightforward, it has never before been possible to test it adequately because representative national surveys are usually carried out only in relatively developed societies. Using the 1990–91 World Values Surveys, we can now test this hypothesis across the full range of economic development. We elsewhere display the relationship between per capita gross national product and scores on the 12-item measure of materialist/postmaterialist values for all 40 societies studied in the 1990–91 World Values Survey (Abramson and Inglehart N.d., fig. 1).[16] As we show, societies with relatively high per capita gross national product tend to have higher proportions of postmaterialists than poorer countries have. There is a strong relationship between per capita gross national product and values. The overall correlation is r = .68, significant at the .0001 level. The regression coefficient indicates that every thousand-dollar increase in per capita gross national product contributes to a 1.7 point gain on the materialist/postmaterialist value index. This test suggests that a linear model fits the data relatively well.[17]

Diez-Nicolas (n.d.) demonstrates that this relationship also holds true at the regional level in Spain. He has included the four-item materialist/postmaterialist values battery in monthly national surveys of the Spanish public since 1988. Cumulating large numbers of interviews for each region enables him to perform a statistically reliable analysis of the relationship between values and economic security at the regional level, in a country with great regional variation. He finds that the relative level of postmaterialism varies considerably from region to region and is stable over time, with the wealthiest regions (the Basque country and Madrid) consistently having the highest proportions of postmaterialists and the poorest regions (Andalusia and Castilla–La Mancha) showing the lowest proportions of postmaterialists in virtually every year. This relationship is strong and shows a particularly good fit with a given region's level of economic development 25 years prior to the survey, during the median respondent's adolescent years (r = .88).

This reinforces the findings from the global array of 40 societies that we have studied (see Abramson and Inglehart n.d.), which also demonstrates that postmaterialist values are closely linked with prosperity. Although the relationship between per capita gross national product and values is cross-sectional, the value-change thesis implies that this configuration reflects a dynamic process in which economic development leads to increasing postmaterialism through a gradual process of intergenerational change. If this theory is correct, we should find a higher proportion of postmaterialists among the younger cohorts than among the old, *provided* that a society has had sufficient economic growth during the past four or five decades so that the younger cohorts experienced significantly greater economic security during their preadult years than did those who are now in their fifties, sixties, or seventies.

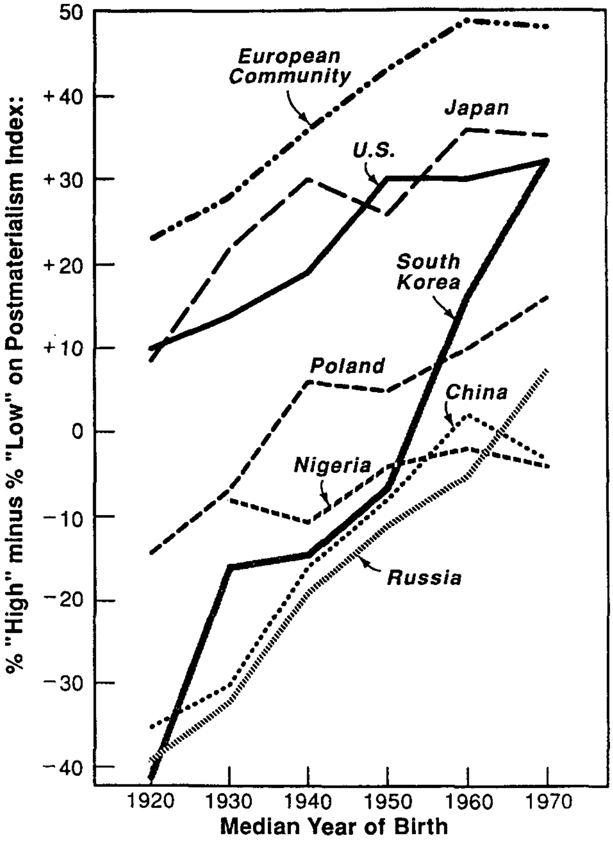
Figure 5 presents the value scores among birth cohorts in several selected societies studied by the 1990–91 World Values Survey (the entire array would be unreadable). As it illustrates, the younger birth cohorts do, indeed, show considerably higher proportions of postmaterialists than the older cohorts. This is not a phenomenon that is limited to Western democracies: it is found across societies having a wide variety of political and economic institutions and a wide variety of cultural traditions. Though the richer countries have much higher absolute proportions of postmaterialists than the poorer ones, we also find a steep slope reflecting intergenerational value differences in poor countries that have experienced a rapid increase in prevailing standards of living during the past several decades.

The results from the European Community countries have already been examined in detail and are combined into a single line on Figure 5. Over all, the European Community shows the highest proportion of postmaterialists on this graph, with the United States and Japan also ranking high. Even the oldest birth cohorts in these advanced industrial societies rank higher than even the youngest cohorts in most other countries. But an upward slope, linked with a rising proportion of postmaterialists to materialists as we move from old to young, is also found in Russia and East Asia.

At first glance, it might seem surprising that the shift from materialist to postmaterialist values has taken place in Russia, which experienced economic stagnation during the decade preceding these surveys and which by 1990 was in a state of economic collapse. If the postmaterialist shift simply reflected

FIGURE 5

Values by Birth Cohort, in Western Democracies, Eastern Europe, and East Asia.



Source: 1990–91 World Values Survey.
Note: Respondents are classified as "high" on the 12-item materialist/postmaterialist values index if they gave high priority to at least three of the five postmaterialist goals (i.e., as one of the two most important goals out of each group of four goals); they are classified as "low" if they chose *none* of the five postmaterialist goals among their high priorities.

recent conditions, we would *not* expect to find intergenerational differences. But Inglehart's theory postulates a long-term process of intergenerational change based on the differences experienced during a given cohort's preadult years. From this perspective, we *would* expect to find sizable intergenerational value differences in Russia, for the formative experiences of the cohorts born in the 1950s, and 1960s, and the 1970s were characterized by far more secure circumstances than those which shaped the cohorts born in the 1920s, 1930s, or 1940s.

Older Russians, for example, experienced the mass starvation linked with forced collectivization during the 1920s, followed by the terror and the Stalinist purges of the 1930s and the starvation and the loss of 27 million lives in the Soviet Union during World War II. The 1950s and 1960s, by contrast, were an era of recovery and rapid economic growth at rates that exceeded those of most Western countries. This was the era that led Khrushchev to boast, "We will bury you" (economically)—and at the time, many Western observers thought it a plausible boast. Recent years have been calamitous, creating a period effect that tends to drive all of the Russian cohorts downward toward the materialist pole. But the formative years of the younger cohorts were far more secure than those of the older cohorts, and if the intergenerational differences reflect differences in preadult experience, we would indeed expect to find sizable evidence of intergenerational change in Russia. We do, as Figure 5 demonstrates. Though the Russian cohort line starts and ends at a level far below that of the richer countries, intergenerational differences in Russia are even steeper than those found in Western Europe, the United States, or Japan. Future research will be needed to determine whether the relatively postmaterialist values of young Russians will persist in the face of the economic collapse following the dissolution of the Soviet Union.

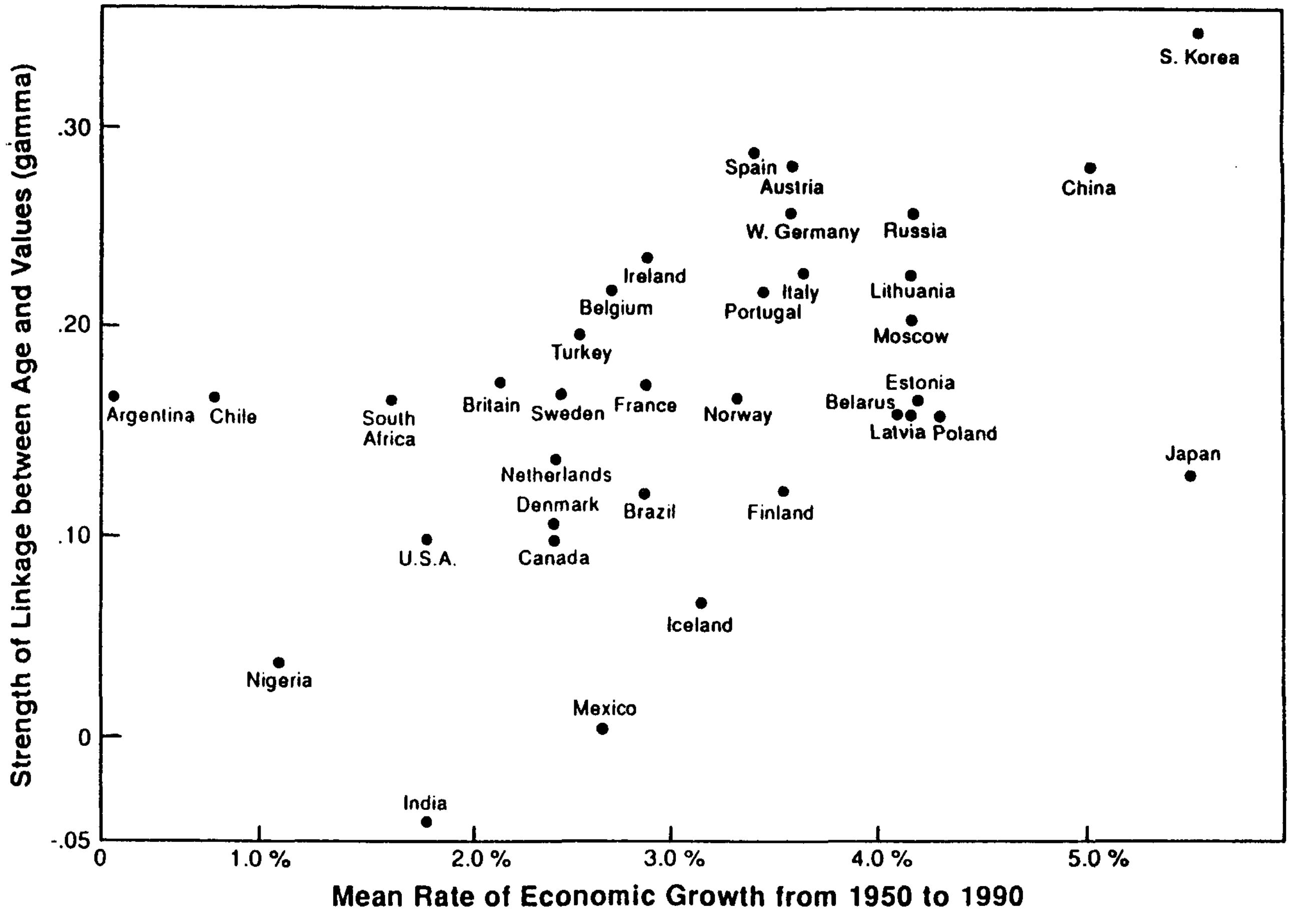
China shows an equally steep slope, reflecting sharp intergenerational differences that may have contributed to the Spring 1989 clash between young intellectuals and an octogenarian leadership still in control of the army. There were massive differences between the formative experiences of the older generation in China, who lived through an era of mass starvation and civil war that went on almost continuously from the 1920s to 1949, and those of the younger cohorts, brought up in conditions of relative stability and prosperity, though broken by the severe but relatively brief upheavals of the Great Leap Forward and the Cultural Revolution in the late 1960s.

The most dramatic set of intergenerational value differences are found in South Korea. Since the 1950s, South Korea has shown spectacular economic growth rates: among all the countries for which the World Bank reports data, only one country (Singapore) showed higher economic growth rate during the period from 1965 to 1990. Apart from this ministate, South Korea has led the world in economic growth, rising from starvation-level poverty in the 1950s to become a relatively rich country. In keeping with this history, older Koreans show an immense preponderance of materialist priorities, and Korea's overall level of postmaterialism is still low. But young South Koreans show a clear preponderance of postmaterialist priorities, with the slope rising so steeply that the youngest cohort actually converges with its American counterparts.

By contrast with South Korea, the Nigerian data show negligible intergenerational value differences: the old are almost as likely to emphasize postmaterialist values as are the young. Here again, the size of the intergenerational value differences reflects the rate of economic change experienced by the given country, for Nigeria has had virtually no increase in per capita income since the 1960s. As our theory implies, intergenerational value differences are present if the formative years of the younger cohorts were shaped by significantly higher levels of economic security

FIGURE 6

Differences between the Values of Young and Old in Societies with Differing Rates of Economic Growth



Note: Data on economic growth were not available for Bugaria, Czechoslovakia, East Germany, Hungary, or Northern Ireland.
Source: 1990–1991 World Value Survey; $r = .52$ $p < .001$; based on the 12-item index of materialist/postmaterialist values.

than those of the older cohorts. In countries that have not experienced economic development, this does not hold true.

Figure 6 condenses the relationship between age and values for each country into a single coefficient. As this figure demonstrates, the selected examples shown in Figure 5 reflect the overall pattern: intergenerational value differences tend to be largest in those countries which have experienced the greatest amounts of economic growth during the past 40 years. Accordingly, the correlation between age and values is strongest in such countries as South Korea and China. It is weak or negligible in such countries as Nigeria and India, which have not only experienced much lower rates of economic growth than China or South Korea but also have much more unequal distributions of income, with the result that substantial proportions of the population live at the edge of starvation.

Note also that the intergenerational value differences are relatively weak in the United States (the four-item index shows even weaker differences): though it has a relatively high absolute *level* of postmaterialism, the rate of intergenerational change is relatively small. Although the United States has been one of the world's richest countries throughout the twentieth century, it has not experienced dramatic *changes* between the formative experiences of younger and older cohorts comparable to those which have characterized Europe and East Asia. The United States has been a relatively rich country throughout the lifetime of virtually everyone in the sample and was not devastated by World War II, but it has had relatively slow growth in recent decades.

We find some deviant cases. Argentina and Chile are "overachievers," showing larger intergenerational differences than their economic growth rates would predict; while Japan is an "underachiever," showing smaller intergenerational value differences than its history of economic growth would predict. But overall, the pattern is clear. High rates of economic growth tend to go with large intergenerational value differences: the relationship is strong (r = .52) and statistically significant at better than .001 level.

Data from a long time series would be needed in order to demonstrate directly that economic development tends to produce an intergenerational shift toward postmaterialist values globally, and such data are not available for most of these countries. But the evidence that *is* available has a remarkably good fit with the predictions of the postmaterialist value shift thesis, and we can supplement it with a modest amount of relevant time series data: for 20 of these countries, data from the four-item values battery are available from both the 1981–83 and the 1990–91 World Values Survey.[18] As Table 5 shows, among these countries, 18 out of 20 reveal a shift in the predicted direction. Only two countries (Iceland and South Africa) show shifts toward materialism. We have no explanation for why Iceland is a deviant case, but it is not at all surprising that South Africa shows a shift toward materialist goals. A society's values at any given time point reflect a combination of long-term trends and current period effects, and South Africa experienced a period of severe insecurity during the 1980s. Its economy, suffering from international boycott and low commodity prices, experienced economic stagnation throughout the 1980s. Moreover, widespread violence and political instability gave rise to growing concern for physical security among both blacks and whites. Powerful period effects were working to produce a sense of insecurity, rather than the security that contributes to postmaterialist values.

TABLE 5

The Shift toward Postmaterialist Values: Results from the 1981–83 versus 1990–91 World Values Surveys

	PERCENTAGE POSTMATERIALIST – PERCENTAGE MATERIALIST		
COUNTRY	1981–83	1990–91	NET SHIFT
Finland	21	23	+2
Netherlands	−2	26	+28
Canada	−6	14	+20
Iceland	−10	−14	−4
Sweden	−10	9	+19
W. Germany	−11	14	+25
Britain	−13	0	+13
France	−14	4	+18
Belgium	−16	2	+18
S. Africa	−16	−33	−17
Mexico	−19	−14	+5
Ireland	−20	−4	+16
Argentina	−20	−6	+14
Norway	−21	−19	+2
United States[a]	−24	6	+30
Japan	−32	−19	+13
Italy	−39	7	+46
Spain	−41	−6	+35
N. Ireland	−45	−7	+38
Hungary	−50	−41	+9

Source: 1981–83 and 1990–91 World Values Surveys.

[a] The values question was not asked in the United States in the 1981–83 survey; results are from the 1980 NES survey. Results are based on the four-item values index.

CONCLUSION

The critics who argue that there has been no clear trend toward postmaterialism are mistaken. Despite Thomassen and van Deth's doubts, the evidence since 1986 makes it clear that there has been a strong trend toward postmaterialism in the Netherlands. Although Shively is technically correct in stating that one cannot definitively reject a life-cycle interpretation using cohort data, the data strongly suggest that generational replacement has been a major long-term force pushing postmaterialism upward. As of 1992, the data for all eight countries they examined reveal a statistically significant trend toward postmaterialism.

Clarke and Dutt also assert that short-term change in values responds to economic changes in ways that are inconsistent with Inglehart's thesis. The argument that postmaterialism increases when unemployment rises is not sustainable. At the individual level, the bulk of the data indicate that persons who are concerned about unemployment tend to have materialist values. During the early 1980s, postmaterialism did tend to increase when unemployment was rising, but this relationship reflects a negative correlation between inflation and unemployment. In fact, in those countries in which the effects of unemployment are statistically significant, unemployment is related to the growth of *materialism*.

In short, the basic four-choice value measure developed by Inglehart behaves very much as he theorized. By the usual tests of validity, the basic four-choice battery has performed remarkably well. Over the past two decades, investigators in more than 20 countries have tested its validity against scores of theoretically relevant demographic and attitudinal variables; in the vast majority of cases, the predicted relationships have been found. The dimensional structure of the measure has also been analyzed in detail, and the measure has a theoretically coherent structure across a wide range of countries. The stability of this structure has also been demonstrated in surveys conducted in 1973, 1978, 1988, and 1990 that employ a larger number of value choices. To a remarkable degree, the same structure appears in survey after survey.

The four-choice indicator that has been used since 1970 has proven to be a very useful indicator of values, and one that is strongly related to intergenerational differences. This relationship has allowed scholars to predict the direction and even the approximate magnitude of change (the component due to intergenerational population replacement). That cohort replacement is the major force driving value change seems clear. Figures 2–4 show that given cohorts tended to have about the levels of postmaterialism in 1992 as they had when they were first surveyed 22 years earlier. If there had been no population turnover during these two decades, overall levels of postmaterialism would have increased very little. But in fact, absolute increases in postmaterialism took place in seven out of the eight countries for which detailed data are available over the past 22 years. In every country but Belgium, generational replacement has been a major force pushing absolute levels of postmaterialism upward.

Of course, there is no guarantee that the trend toward postmaterialism will continue. As Duncan reminded us, "There is nothing about a trend, supposing it to have been reliably ascertained for some specific period, that guarantees its own continuation" (1968, 679). It does seem likely, at least for the next several decades, that future generational replacement will tend to push postmaterialism upward. In every society except India, the young were more likely to have postmaterialist values than their elders (although, as Figure 6 shows, this relationship varies greatly from society to society). As Hyman observed: "It is the inevitable fact of life that the young will replace the old and will determine the future course of any trend. Thus if the young differ from the old, and were to continue to do so despite their own aging, some prediction can be ventured" (1972, 243).

Although generational replacement is likely to push postmaterialism upward, our time series analyses demonstrate that economic forces (especially changing inflation rates) can affect value change. Thus, while a trend has been clearly documented and although young Europeans have remained more postmaterialist despite their own aging, economic forces can influence the pace, and even the direction, of future value change. It is possible to make projections about the future magnitude of value change in these societies (see Abramson and Inglehart 1987, 1992; Inglehart 1990), but such projections are only guidelines about what *might* happen.

The consequences of the breakup of the Soviet Union are only beginning to be felt, and it is easy to advance a bleak scenario that renders the entire study of mass values as meaningless. But it is possible to end on a brighter note. We now have data on values from a wide range of societies, representing 70% of the world's population. Conducting surveys, in cooperation with native scholars would have been impossible in many of these countries a decade earlier. The changes that made such research possible have benefited social scientists, but they have the potential of benefiting billions of ordinary citizens as well. This broad range of data from the World Values Surveys helps place the Western European data in global perspective. These new data illustrate two major points.

First, the shift from materialist to postmaterialist values is not a uniquely Western phenomenon. Rather, it is found in societies with widely different institutions and cultural traditions. The rise of postmaterialist values is closely linked with prosperity and seems to occur wherever a society has experienced enough economic growth in recent decades so that the younger birth cohorts have experienced significantly greater economic security during their formative years than older cohorts experienced. Intergenerational value differences reflect a society's rate of economic growth. Economic growth, of course, is only one factor contributing to security or insecurity, but it happens to be (1) an important part of the story and (2) one for which we have relatively good cross-national and cross-time data. War, domestic upheaval, and ethnic conflict can also have a major impact on feelings of security, but precisely because they tend to be situation-specific (and are less readily quantified), they are more difficult to analyze empirically.

Second, where value change has occurred, intergenerational differences are remarkably robust. In Western Europe, clear and substantial differences between the values of younger and older birth cohorts persisted through the recessions of the mid-1970s and the early 1980s. More remarkable still, in Russia and Eastern Europe sizable intergenerational value differences are found despite the collapse of the economic and political systems in recent years. These values show predictable period effects, in response to current economic conditions. But the postmaterialist value shift does not simply reflect current conditions: it also has a long-term component that seems to reflect the distinctive formative circumstances that given birth cohorts experienced as much as 40 or 50 years ago.

The enduring intergenerational differences found here seriously undermine rational choice explanations that seek to explain behavior as an immediate response to a given situation, unshaped by internal cultural differences, for we observe generational differences that seem to reflect the enduring legacy of the distinctive formative experiences of given generations. At any given point in time, the respective birth cohorts in a given society are in the same situation as their elders, but they respond to it in fundamentally different ways, because they evaluate it by different values. These different responses to the same situation do not seem simply to reflect the fact that the respective cohorts are of different ages, for the distinctive values continue to characterize given cohorts even after they have aged over many years.

Notes

We are grateful to the Commission of the European Communities (and especially to Karlheinz Reif and Anna Melich) for providing us with the Euro-Barometer surveys and to Jim Granato and to Charles W. Ostrom for their extensive advice. Sara McLaughlin assisted with the data analysis. Jan van Deth, Nicolas van de Walle, and Frank P. Zinni, Jr., made helpful comments on earlier versions of this article. The U.S.

National Election Studies surveys were provided by the Inter-University Consortium for Political and Social Research.

1. Respondents are asked to select what they believe their country's two top goals should be among the following four choices: (1) maintaining order in the nation, (2) giving the people more say in important government decisions, (3) fighting rising prices, (4) protecting freedom of speech. Respondents who select "maintaining order" and "fighting inflation" are classified as materialists, while those who choose "giving the people more say" and "freedom of speech" are classified as postmaterialists. The four remaining combinations are classified as "mixed."

2. As the basic measure of materialism/postmaterialism has three categories (materialist, mixed, and postmaterialist), the percentage difference index is equivalent to a mean score (see Inglehart 1990, 436). More specifically, it is equivalent to a mean score that assigns each postmaterialist a score of 100, each respondent with mixed values a score of 0, and each materialist a score of −100. In the vast majority of analyses, there is a monotonic relationship among the three categories of this measure. For example, in the 1992 U.S. presidential election postmaterialism was positively correlated with voting for Bill Clinton. According to our analyses of the 1992 National Election Study survey, among major-party voters who were postmaterialists (N = 248), 76% voted for Clinton; among those with "mixed" values (N = 876), 57% voted for Clinton; and among major-party voters who were materialists (N = 212), 43% voted for Clinton (based upon weighted *N*s.)

3. As the time of writing, the Inter-University Consortium for Political and Social Research survey data archive had released the results of all of the Euro-Barometer surveys conducted through Fall 1992.

4. Beginning with Fall 1990, the Euro-Barometer surveys include the newly formed states from the territory that used to comprise East Germany. To maintain overtime comparability, we exclude respondents from these states from our analyses.

5. Following Inglehart (1977), in our analysis of change over time, we combined the results of European Community Surveys conducted in 1970 and 1971. For 1977, 1978, and 1980–92, we combined the results of two surveys. Clarke and Dutt also employ a single annual result for their analyses (1991, Figs. 1, 2), although Thomassen and van Deth provide results for each semiannual survey (1989, fig. 1).

6. We give the full distribution of values for each of these eight European countries for each survey year from 1970–71 or 1973 through Spring 1990 in Abramson and Inglehart 1992.

7. In addition to choosing among the four standard national goals (see n. 1), respondents are also asked to choose their two top goals among the following two sets:

1. maintain a high rate of economic growth
2. make sure that this country has strong defense forces
3. give people more say in how things are decided at work and in their community
4. try to make our cities and countryside more beautiful

and

1. maintain a stable economy
2. fight against crime
3. move toward a friendlier, less impersonal society
4. move toward a society where ideas count more than money.

For the scoring procedures used to create the twelve-choice index, see Inglehart 1990.

8. We present complete cohort results for each of the eight countries through Spring 1990, as well as the specific values for each cohort for the combined six-nation sample in Abramson and Inglehart 1992. As we point out, most of our cohort estimates for the six-nation sample are based on over a thousand cases, and even the smallest cohorts contain over six hundred cases.

9. The impact of education on values is complex, and we discuss its multifaceted effects in Abramson and Inglehart n.d. We present an extensive critique of Duch and Taylor's (1993) argument that past educational attainment and current economic conditions are the major factors influencing values.

10. See n. 1.

11. The cohort results track cohorts through Spring 1992. Data about inflation and unemployment are based upon statistics published by the Statistical Office of the European Communities. German economic statistics for 1990–92 exclude the states that used to comprise East Germany.

12. Chatfield points out that the conventional tests of autocorrelation lack power when N is less than 100 (1989, 62). He suggests that the most powerful test is to examine the first-order autocorrelation function coefficient. As there are 18 observations for Denmark and Ireland, a finding of +/−.471 indicates the presence of autocorrelation; as there are 19 observations for the other countries and for the six-nation sample, a finding of +/−.458 indicates autocorrelation. None of the first-order autocorrelation functions is close to these magnitudes. The highest first-order autocorrelation function is in Germany (.263). Germany is the only country in which a result is more than marginally affected by controlling for autocorrelation.

13. Even though there was no problem with autocorrelation, we removed the minimal effects of autocorrelation using iterative Paris–Winsten estimators. (See Ostrom 1990 for a discussion.) We present these results in Inglehart and Abramson 1993, tbl. A1. Except for changing a significance level in Germany (the significance of the relationship between unemployment and values falls from .0616 to .1334), there are only marginal differences between the results in that table and those in our Table 4.

14. Clarke and Dutt (1991) also argue that Inglehart's measure of materialism/postmaterialism manifests a high level of individual-level instability. This argument is orthogonal to our present analysis, since even relatively high levels of individual-level instability are compatible with the thesis that long-term generational replacement tends to drive postmaterialism upward. However, we have argued elsewhere that Clarke and Dutt's estimates are unreliable: LISREL analyses by Inglehart (1990), De Graaf (1988) and van Deth (1989) all indicate higher levels of stability than Clarke and Dutt found. We offer an extensive discussion of alternative tests of individual-level stability in Inglehart and Abramson 1992.

15. The data for the 1981–83 World Values Survey are available through the ICPSR survey data archive; and the 1990–91 World Values Survey data are currently being documented for release through the ICPSR archive and are scheduled to be released in 1994. The documentation will contain information about the sampling procedures used in all 40 societies.

16. Factor analyses demonstrate that some minor modifications in Inglehart's 12-choice measure are necessary to construct a measure that is cross-culturally comparable across a wide variety of societies (see Abramson and Inglehart 1993). The "economic growth" option tends to load as a postmaterialist value in the state-socialist countries (including China), and the "beautiful cities" choice tends to load as a materialist value in several state-socialist countries and in Third World societies. However, five of the six items designed to tap postmaterialist values (all except "beautiful cities") clearly tap postmaterialist values. Respondents are initially scored from zero to five, according to the total number of postmaterialist responses they select. Although we have developed a percentage difference to facilitate the visual presentation of these data here, we display the full distribution of scores for each country in Inglehart and Abramson, 1993, tbl. A2.

17. Inglehart (1987) does discuss the diminishing marginal utility of increased wealth. However, he does not argue that increasing wealth has a diminishing marginal impact on feelings of security, and the linear relationship between wealth and postmaterialism is consistent with Inglehart's theory.

18. Although the first World Values Survey measured materialism/postmaterialism in Denmark, technical problems render this measurement noncomparable with the second World Values Survey. At the request of the Argentine investigators, the 1983 survey of Argentina was not released through the ICPSR.

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American Political Science Review Vol. 88, No. 2 June 1994

TOWARD A THEORY OF CONSTITUTIONAL AMENDMENT

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Constitutional design proceeds under the assumption that institutions have predictable consequences, but modern political science has not pursued the empirical verification of these predicted consequences with much vigor. I shall attempt to link the theoretical premises underlying one important aspect of constitutional design, the amendment process, with the empirical patterns revealed by a systematic, comparative study of constitutions. An examination of all amendments in the 50 American states since 1776 reveals patterns that are then confirmed using data from 32 national constitutions. The interaction of the two key variables affecting amendment rate can be described by an equation that generates predicted amendment rates close to those found in the cross-national empirical analysis. A constitution's length measured in number of words, the difficulty of an amendment process, and the rate of amendment turn out to have interlocking consequences that illuminate principles of constitutional design.

A constitution may be modified by means of (1) a formal amendment process, (2) periodic replacement of the entire document, (3) judicial interpretation, and (4) legislative revision. What difference does it make whether we use one method rather than another? What is the relationship between these four methods? What do we learn about the constitutional system and its underlying political theory by the pattern of choice among these alternatives? These are some of the questions to be addressed.

Although it is true that a constitution is often used as ideological window dressing and that even in places where constitutions are taken very seriously these documents fail to describe the full reality of an operating political system, it is also true that today hardly any political system, dictatorial or democratic, fails to reflect political change in its constitution. Constitutions may not describe the full reality of a political system, but when carefully read they are windows into that underlying reality.

This essay is an initial attempt to use a critical, if often overlooked, constitutional device—the amendment process—as a window into both the reality of political systems and the political theory or theories of constitutionalism underlying them. A good deal has been written about the logic of constitutional choice using rational-actor models, but little has been written about the empirical patterns that result from constitutional choice. The classical example of the first approach is the work of James Buchanan and Gordon Tulloch (1965). The second approach is exemplified by the work of Douglas W. Rae (Rae 1967; but see also Grofman 1986; Przeworski 1991; Shugart and Carey 1992). I shall use the latter method and attempt to be systematic, comparative, and, to the extent possible, empirical. I shall begin with a brief overview of the theoretical assumptions that underlay the formal amendment process when it was invented, identify a number of theoretical propositions concerning the amendment process, and then look for patterns in the use of the amendment process that can be used to create empirical standards upon which to erect a theory of constitutional amendment useful to those engaged in constitutional design.

THE ORIGINAL PREMISES UNDERLYING THE AMENDMENT PROCESS

The modern written constitution, first developed in English-speaking North America, was grounded in a doctrine of popular sovereignty (Adams 1980; Morgan 1988). Even though many in Britain were skeptical at best, Americans did not regard popular sovereignty as a experimental idea but, rather, as one that stood at the very heart of their shared political consensus (Lutz 1988, esp. chap. 7). American political writing had used the language of popular sovereignty before Locke's *Second Treatise* was published, and the early state constitutions of the 1770s contained clear and firm statements that these documents rested upon popular consent (Lutz 1980, 218–25). Although the theory of popular sovereignty was well understood in America by 1776, the institutional implications of this innovative doctrine had to be worked out in constitutions adopted over the next decade. Gradually, it was realized that a doctrine of popular sovereignty required that constitutions be written by a popularly selected convention, rather than the legislature, and then ratified through a process that elicited popular consent—ideally, in a referendum. This double implication was established in the process used to frame and adopt the 1780 Massachusetts and 1784 New Hampshire constitutions, although the referendum portion of the process did not become standard until the nineteenth century.

Americans moved quickly to the conclusion that if a constitution rested on popular consent, then the people could also replace it with a new one. John Locke had argued that the people could replace government but only when those entrusted with the powers of government had first disqualified themselves by endangering the happiness of the community to such a degree that civil society could be said to have reverted to a state of nature. Americans went well beyond Locke by institutionalizing the power to change the constitution amid civil society—that is to say, whenever they wanted. It is of considerable importance that this included not only replacing the constitution but also formally amending it.

The first new state constitution in 1776, that of New Jersey, contained an implicit notion of amendment, but the 1776 Pennsylvania document contained the first explicit amendment process—one that used a convention process and bypassed the legislature.[1] By 1780, almost half the states had an amendment procedure, and the principle that the fundamental law could be altered piecemeal by popular will was firmly in place.

In addition to popular sovereignty, the amendment process was based on three other premises central to the American consensus in the 1770s: an imperfect but educable human nature, the efficacy of a deliberative process, and the distinction between normal legislation and constitutional matters. The first premise, clearly explicated by Vincent Ostrom (1987), held that humans are fallible but capable of learning through experience. Americans had long considered each governmental institution and practice to be in the nature of an experiment. Since fallibility was part of human nature, provision had to be made for altering institutions after experience revealed their flaws and unintended consequences. Originally, therefore, the amendment process was predicated not only on the need to adapt to changing circumstances but also on the need to compensate for the limits of human understanding and virtue. In a sense, the entire idea of a constitution rests on an assumption of human fallibility, since, if humans were angels, there would be no need to erect, direct, and limit government through a constitution.

A belief in the efficacy of a deliberative process was also part of the general American constitutional perspective. A constitution was viewed as a means not merely to make collective decisions in the most efficient way possible but to make the *best possible* decisions *in pursuit of the common good* under a condition of *popular sovereignty*. The common good is a more difficult standard to approximate than the good of one class or part of the population, and the condition of popular sovereignty, even if operationalized as a system of representation, requires the involvement of many more people than forms of government based on other principles. This in turn requires a slow, deliberative process for any political decision, and the more important the decision, the more deliberative the process should be. Constitutional matters were considered more important in 1789 America than normal legislation, which led to a more highly deliberative process distinguishing constitutional from normal legislative matters. The codification of the distinction in constitutional articles of ratification and amendment resulted in America constitutions being viewed as higher law that should limit and direct the content of normal legislation.

Popular sovereignty implies that all constitutional matters should be based upon some form of popular consent, which in turn implies a formal, public process. Human fallibility implies the need for some method of altering or revising the constitution. A distinction between normal and constitutional matters implies that constitutional matters require a distinctive, highly deliberative process and thus implies the need for an amendment procedure more difficult than that used for normal legislation.

Together these premises require that the procedure be neither too easy nor too difficult. A process that is too easy, not providing enough distinction between constitutional matters and normal legislation, thereby violates the assumption of the need for a high level of deliberation and debases popular sovereignty while one that is too difficult, interfering with the needed rectification of mistakes, thereby violates the assumption of human fallibility and prevents the effective utilization of popular sovereignty.

The literature on constitutions at one time made a distinction between major and minor constitutional alterations by calling the former "revisions" and the latter "amendments." As Albert L. Sturm (1970) points out, the distinction turned out in practice to be conceptually slippery, impossible to operationalize, and therefore generally useless. Because *revision* is used in the literature to mean several different things, I shall use *amendment* as a description of the *formal* process developed by the Americans and *alteration* to describe processes that instead use the legislature or judiciary. Unless we maintain the distinction between formal amendment and other means of constitutional modification, we will lose the ability to distinguish competing constitutional theories.

The innovation of an amendment process, like the innovation of a written constitution, has diffused throughout the world to the point where less than 4% of all national constitutions lack a provision for a formal amending process (Maarseveen and Van der Tang 1978, 80). However, the diffusion of written constitutions and the amendment idea do not necessarily indicate widespread acceptance of the principles that underlie the American innovation. In most countries with a written constitution popular sovereignty and the use of a constitution as a higher law are not operative political principles. Any comparative study of the amendment process must first distinguish true constitutional systems from those that use a constitution as window dressing and then recognize that among the former there are variations in the amendment process that rest on assumptions at odds with those in the American version. Indeed, my chief concern is the efficiency with which study of the amending process reveals such theoretical differences.

At the same time, a comparative study of amendment processes allows us to delve more deeply into the theory of constitutional amendment as a principle of constitutional design. For example, we might ask the question, what difference does it make if constitutions are formally amended through a political process that does not effectively distinguish constitutional matters from normal legislation? Why might we still want to draw a distinction between formal amendment and alteration by normal politics as carefully and as strongly as possible? One important answer to the question is that the three prominent methods of constitutional modification other than complete replacement—formal amendment, legislative revision, and judicial interpretation—reflect declining degrees of commitment to popular sovereignty, and the level of commitment to popular sovereignty may be a key attitude for defining the nature of the political system.

BASIC ASSUMPTIONS AND PROPOSITIONS

Every theory has to begin with a number of assumptions. We have seen how the original American version rested on the premises of popular sovereignty, an imperfect but educable human nature, the efficacy of a highly deliberative decision-making process, and the distinction between normal and constitutional law. While these help define the working assumptions of one theory of amendment (albeit the original one), they do not provide a complete basis for describing either the American theory or a general theory of amendment. I turn now to developing a theory that includes the American version but also provides the basis for analyzing any version of constitutional amendment. The intent of the analysis is to provide guidelines for constitutional design in any context—guidelines that will allow framers to link the design of a formal amendment process securely to desired outcomes.

My first and second working assumptions have to do with the expected change that is faced by every political system and with the nature of a constitution, respectively.

Assumption 1. *Every political system needs to be modified over time as a result of some combination of (1) changes in the environment within which the political system operates (including economics, technology, foreign relations, demographics, etc.); (2) changes in the value system distributed across the population; (3) unwanted or unexpected institutional effects; and (4) the cumulative effect of decisions made by the legislature, executive, and judiciary.*

Assumption 2. *In political systems that are constitutional, in which constitutions are taken seriously as limiting government and legitimating the decision-making process they describe, important modifications in the operation of the political system need to be reflected in the constitution.*

If these two assumptions are used as premises in a deductive process, they imply a conclusion that stands as a further assumption.

Assumption 3. *All constitutions require regular, periodic modification, whether through amendment, judicial or legislative alteration, or replacement.*

Alteration (as noted earlier) refers to changes in a constitution through judicial interpretation or legislative action. However, I am initially more concerned with the use of a formal amendment process. *Amendment rate,* a key concept, refers to the average number of formal amendments passed per year since the constitution came into effect. As Albert L. Sturm (1970) points out, many scholars criticize constitutions that are much amended. However, constitutionalism and the logic of popular sovereignty are based on more than simplicity and tidiness. Any people who believe in constitutionalism will amend their constitution when needed, as opposed to using extraconstitutional means. Thus, a moderate amendment rate will indicate that the people living under it take their constitution seriously. The older a constitution is, under conditions of popular sovereignty, the more successful it has been but also the larger the number of amendments it will have. However, it is the *rate* of amendment that is important in this regard, not the total number of amendments.

A successful constitutional system would seem to be defined by a constitution of considerable age that has a total number of amendments which, when divided by the constitution's age in years, represents a moderate amendment rate—one that is to be expected in the face of inevitable change. A less-than-successful constitutional system will have a high rate of constitutional replacement.

This raises the question of what constitutes a "moderate" rate of amendment. Since I hope to illuminate the question empirically, rather than in an a priori manner, I must initially use a symbolic stand-in for "moderate rate of amendment." Since a moderate rate is likely to be a range of rates, rather than a single one, the symbol will define boundaries such that any document with an amendment rate above or below its limits will have an increasing probability of being replaced or an increasing probability that some extraconstitutional means of constitutional evolution is being used. I shall use <#> to represent this moderate range of amendment rates symbolically.

The first proposition is frequently found in the literature, but it has never been systematically verified, or its effect measured.

Proposition 1. *The longer a constitution is (the more words it has), the higher its amendment rate, and the shorter a constitution, the lower its amendment rate.*

Commentators frequently note that the more provisions a constitution has, the more targets there are for amendment and the more likely that it will be targeted because it deals with too many details that are subject to change. While this seems intuitively correct, the data that are used usually raise the question, Which comes first: the high amendment rate or the long constitution? This is because a constitution's length is usually given as of a particular year, rather than in terms of its original length. Is a constitution long because it had a high amendment rate, or did it have a high amendment rate because it was long to begin with?

My second proposition is also a common one in the literature, although it too has never been systematically tested before.

PROPOSITION 2. *The more difficult the amendment process, the lower the amendment rate, and the easier the amendment process, the higher the amendment rate.*

As obvious as this proposition is, it cannot be tested until one shifts from the number of amendments in a constitution to its amendment rate and until one develops an index for measuring the degree of difficulty associated with an amendment process. I shall present such an index as part of what is needed to develop a way of predicting the likely consequences of using one amendment process versus another.

The literature on American state constitutions generally argues that these documents are much longer than the national constitution because they must deal with more governmental functions. For example, if a constitution deals with matters like education, criminal law, local government, and finances, it is bound to be more detailed, longer and thus have a higher amendment rate than one that does not address these matters. From this, I generalize to the following proposition.

PROPOSITION 3. *The more governmental functions dealt with in a constitution, the longer it will be and the higher its rate of amendment will be.*

Constitutions are usually replaced for one of three reasons: (1) a regime change may leave the values, institutions, and/or implications of the old constitution seriously at odds with those preferred by the people now in charge; (2) the constitution may fail to keep up with the times; and (3) the old constitution may have been changed so many times that it is no longer clear what lies under the encrustations, so that clarity demands a new beginning. A moderate amendment rate is an antidote to all three.

PROPOSITION 4. *The further the amendment rate is from the mean of <#>, either higher or lower, the greater the probability that the entire constitution will be replaced and thus the shorter its duration. Conversely, the closer an amendment rate is to the mean of <#>, the lower the probability that the entire constitution will be replaced and thus the longer its duration.*

A low rate of amendment in the face of needed change may lead to the development of some extra-constitutional means of revision—most likely, judicial interpretation—to supplement the formal amendment process. I can now, on the basis of earlier discussion, generate several propositions that will prove useful toward the end of my discussion on the implications of the major competing forms of formal constitutional amendment.

PROPOSITION 5. *A low amendment rate associated with a long average constitutional duration strongly implies the use of some alternate means of revision to supplement the formal amendment process.*

PROPOSITION 6. *In the absence of a high rate of constitutional replacement, the lower the rate of formal amendment, the more likely the process of revision is dominated by a judicial body.*

PROPOSITION 7. *The higher the formal amendment rate, (a) the less likely that the constitution is being viewed as a higher law, (b) the less likely that a distinction is being drawn between constitutional matters and normal legislation, (c) the more likely that the document is being viewed as a code, and (d) the more likely that the formal amendment process is dominated by the legislature.*

PROPOSITION 8. *The more important the role of the judiciary in constitutional revision, the less likely the judiciary is to use theories of strict construction.*

I shall test propositions 1–4 using data from the American state constitutions and then seek further verification by examining the amendment process in nations where constitutionalism is taken seriously and does not serve merely as window dressing. The American state documents are examined first because data on them are readily available and easily comparable, because the similarities in their amendment process reduce the number of variables that must be taken into account, and because together they comprise a significant percentage of human experience with serious constitutionalism.

AMENDMENT PATTERNS IN AMERICAN STATE CONSTITUTIONS, 1776–1991

Albert L. Sturm (1970) summarizes the literature as seeing state constitutions burdened with (1) the effects of continuous expansion in state functions and responsibilities and the consequent growth of governmental machinery; (2) the primary responsibility for responding to the increasing pressure of major problems associated with rapid urbanization, technological development, population growth and mobility, economic change and development, and the fair treatment of minority groups; (3) the pressure of special interests for constitutional status; and (4) continuing popular distrust of the state legislature, based on past abuses, which results in detailed restrictions on governmental activity. All of these factors contribute to the length of state constitutions, and it is argued that not only do these pressures lead to many amendments—and thus to greater length—but that greater length itself leads to the accelerated need for amendment simply by providing so many targets for change. Thus, length becomes a surrogate measure for all of these other pressures to amend and is a key variable.

Appendix table A-1 shows basic data for duration, length, and amendments for the U.S. constitution and the constitutions of the 50 states. The average amendment rate is much higher for the state constitutions than it is for the U.S. Constitution. Between 1789 and 1991 the Constitution was amended 26 times for a rate of .13 (26 amendments/202 years = .13 amendments per year). As of 1991, the current state constitutions had been in effect for an average of 95 years and had been amended a total of 5,845 times, or an average of 117 amendments per state. This produces an average amendment rate of 1.23 for the states, about 9 1/2 times the national rate.

Proposition 1 hypothesizes a positive relationship between the length of a constitution and its amendment rate: the longer a constitution when adopted, the higher its rate of amendment. The data on American state constitutions strongly support proposition 1 with a correlation coefficient of .6249 significant at the .001 level. Furthermore, the relationship holds whether we use the original or the current amended length.

The average length of state constitutions increases from about 19,300 words as originally written to about 24,300 as amended by 1991, which raises the interesting question of what difference it makes whether we use a constitution's original length or its current amended length. The surprising answer is that it makes no real difference. The curve of best fit for amendment rates using the original length of a constitution has a slope of .58, that of amendment rates using the amended length, a slope of .62. There is thus good reason, when testing the propositions against foreign national constitutions, for using *either* the original or the amended length.

Also, the correlation coefficient between amended and unamended rates is .9936 (significant at the .001 level), which strongly implies that the rate of increase in amendment rate resulting from increasing a constitution's length is virtually constant across all lengths. Finally, since at any point in time the set of constitutions used to test the propositions will vary considerably in age and thus be a mixture of documents ranging from slightly amended to highly amended, we should probably use a composite curve that reflects this inevitable mix. In the case of American state constitutions, the obvious composite curve would be one that averaged .58 and .62. The resulting amendment rate curve with a slope of .60 indicates that for every ten-thousand-word increase in a constitution's length, the amendment rate will increase by .60.

The relationship between the length of a constitution and its amendment rate is the strongest and most consistent one found in the analysis of data drawn from the American states. The strength of this relationship can be underscored by a partial listing of the variables examined that did not show any significant independent correlation with amendment rate. These variables include geographical size, population, level of industrialization, per capita personal income, per capita state expenditures, size of legislature, partisan division in legislature, geographical region, geographical proximity, and the historical era in which the constitution was written. Controlling for these other variables, the importance of constitutional length remains, whereas controlling for constitutional length, the few weak correlations with these other variables disappear.

State constitutions, on average, are much longer than the U.S. Constitution. Can we account for this difference? Proposition 3 suggests that the wider range of governmental functions at the state level results in significantly longer documents and thus produces a higher amendment rate that makes them longer still (in line with proposition 1).

Data from a recent decade show that amendments dealing with local governmental structure (4.7%), state and local debt (4.3%), state functions (9.0%), taxation and finance (14.1%), amendment and revision (2.6%), and local issues (28%) comprise about 63% of all state amendments and pertain to topics that have not been part of national constitutional concern.[2]

If we exclude these categories of issues from the amendment count, we end up with an adjusted state rate of about .47. This figure is still a bit over three-and-a-half times the national amendment rate, but by eliminating the amendments peculiar to state constitutions we obtain a figure for comparison with the national rate (.13), using what amounts to the same base. The difference between .13 and .47 represents what we might term the "surplus rate" that still needs to be explained. An interesting question—one that never seems to be asked—is whether the state amendment rate is too high or the national amendment rate is too low.

The answer depends in part on one's attitude toward judicial interpretation. Propositions 5 and 6 suggest that for one who prefers judicial interpretation as a means of modifying a constitution over a formal amendment process, the amendment rate for the national document is not too low. However, for one who prefers a formal amendment process, such as an attachment to popular sovereignty, the amendment rate of the U.S. Constitution may well be too low and the amendment rate of the states is to be preferred.

Propositions 5 and 6 assume a low rate of amendment coupled with constitutional longevity. Proposition 4, on the other hand, posits a general relationship between the rate of amendment and constitutional longevity. Dividing the number of constitutions a state has had into the number of years it has been a state produces the average duration of the state's constitutions—a measure of constitutional activity that controls for a state's age. Table 1 shows that a high amendment rate is associated with low average duration and, thus, high replacement rate (r = −.3561, significant at the .01 level).

However, proposition 4 predicts that the rate at which constitutions are replaced will increase as the amendment rate moves up *or* down with respect to <#>. In Table 1, the amendment rate is the dependent variable. However, if we make it the indepen-

TABLE 1

Amendment Rate of a State Constitution, by Average Duration

	AVERAGE DURATION (YRS.)						
	1–25	26–50	51–75	76–100	101–25	126–50	151+
Amendment rate	2.37 (3)	1.95 (13)	1.26 (13)	1.10 (6)	.93 (8)	.84 (5)	.64 (2)

Note: The numbers in parentheses indicate the numbers of constitutions in that range of average duration.

dent variable instead, we can test directly for the bidirectional effect. Table 2 supports proposition 4. The average duration of a state's constitution declines as the amendment rate goes above 1.00 and as it goes below .75. This means that for American state constitutions, an amendment rate between .75 and 1.00 is associated with the longest-lived constitutions and thus with the lowest rate of constitutional replacement. This range, then, will be defined as <#>. The 13 constitutions with amendment rates within <#> (as just defined) average .89, which we will define as # within <#>.

I turn now to developing an index with which to measure the difficulty of a given amendment procedure. I shall then be ready to look at the constitutions of other nations.

AMENDMENT PATTERNS AND THE CHARACTERISTICS OF THE AMENDMENT PROCESS

In the American states the method of ratifying an amendment can essentially be held constant since every state but one now uses a popular referendum for approval. However, amendments may be initiated by the state's legislature, an initiative referendum, a constitutional convention, or a commission. It is generally held that the more difficult the process of initiation, the fewer the amendments proposed and thus the fewer passed. It is also believed that the initiative has made the process of proposing an amendment too easy and opened a floodgate of proposals that are then more readily adopted by the electorate that initiated them. Another widely held belief is that the stricter or more arduous the process a legislature must use to propose an amendment, the fewer the amendments proposed.

TABLE 3

Method of Initiation and State Amendment Rate, 1970–79

RATE AND FREQUENCY OF AMENDMENT	METHOD OF INITIATION		
	PROPOSED BY LEGISLATURE	POPULAR INITIATIVE	SPECIAL CONVENTION
Amendment Rate	1.24	1.38	1.26
% of Amendments Using This Method	91.5	2.2	6.3
No. of Constitutions in Category[a]	50	17	5

Source: Data are derived from Sturm 1982, 78–79.
[a] Total exceeds 50, since many states specify the possibility of more than one method for proposing amendments.

First of all, as Table 3 shows, during a recent decade, relatively few amendments were proposed by other than a legislature. One-third of the states use popular initiative as a method of proposing amendments, and yet in these states the legislative method was greatly preferred. The popular initiative has received a lot of attention, especially in California, but in fact it has had a minimal impact so far.

What has been the relative success of these competing modes of proposing constitutions? The relatively few amendments proposed through popular initiative have a success rate roughly half that of the two prominent alternatives (32% versus 64% for legislature initiated and 71% for convention initiated). The popular initiative is in fact more difficult to use than legislative initiative and results in proposals that are less well considered and thus less likely to be accepted.

What about the varying methods for *legislative* initiation? States differ in how large a legislative majority is needed for a proposal to be put on the ballot, and some states require that the majority be sustained in two consecutive sessions. Table 4 summarizes what we find in this regard.

TABLE 2

Average Duration of a State Constitution by Amendment Rate

	AMENDMENT RATE							
	0–.5	.51–.75	.76–1.00	1.01–.25	1.26–.50	1.51–.75	1.76–2.00	2.00+
Average duration	71 (7)	90 (8)	100 (13)	86 (4)	79 (4)	57 (6)	40 (1)	38 (7)

Note: The numbers in parentheses indicate the number of states falling into this range of amendment rate.

TABLE 4

Comparative Effect of Majority Size on Amendment Rate in American State Constitutions

	REQUIRED LEGISLATIVE MAJORITY					
	50% + 1	50% + 1 TWICE	60%	67%	75%	67% TWICE
Ratio of difficulty to simple maj.	1.00 (11)	1.04 (6)	1.26 (9)	1.62 (19)	1.83 (1)	3.56 (4)

Note: The numbers in parentheses indicate the numbers of states using this required legislative majority.

In this table, the decline in the amendment rate produced by each type of legislative majority has been normed against that of the least difficult method. This norming is accomplished by taking the success rate of amendment proposals initiated by a simple (bicameral) legislative majority, and dividing it by the success rate of proposals initiated by a two-thirds legislative majority, a three-fourths majority, etc.—always keeping the other variables constant. For example, the data indicate that in the American states, when the method of initiation is stiffened to require approval by a simple (bicameral) legislative approval *twice,* the amendment rate is reduced from the baseline of 71% to a little over 68%. Dividing 71% by 68% results in an index score of 1.04. Likewise, a requirement for a three-fifths (bicameral) legislative majority results in a success rate of 56%. Dividing 71% by 56% produces an index score of 1.26. A score of 2.00 therefore indicates a method that is twice as difficult, a 3.00 indicates a method three times as difficult, and so on. Table 4 arrays the empirical results from lowest to highest level of difficulty rather than according to any theoretical prediction. The results are mostly in line with common sense expectations, (although why a second majority vote has so little effect while a second two-thirds vote has so much is not clear).

We can derive three conclusions from Table 4:

1. Generally speaking, the larger the legislative majority required for initiation, the fewer the amendments proposed and the lower the amendment rate.
2. Requiring a legislature to pass a proposal twice does not significantly increase the difficulty of the amendment process if the decision rule is one-half plus one.
3. The most effective way to increase the difficulty of amendment at the initiation stage is to require the approval of two consecutive legislatures using a two-thirds majority each time.

Beyond these three interesting proposals, it is also useful to discover that the variance in the degree of difficulty between alternative legislative majorities is sufficient to establish the core of an *index of difficulty* for any amendment process. An attempt at such an index is presented in Appendix B, which identifies sixty-eight possible actions that could in some combination be used to initiate and approve constitutional amendments, and that together cover the combinations of virtually every amendment process in the world. The state data on which Table 4 is based generate the index scores for actions 14 through 23, rounding off the score to the nearest .05. If we assume that legislative processes for approval are symmetrical with those for initiation, the index scores for actions 50 through 59 are the same as those for 14–23. If we assume that a unicameral legislative process is one half as difficult as a bicameral one, the index scores for actions 4–13 and 39–49 are as indicated. As reported earlier, amendments proposed by popular initiative have almost exactly one-half the success rate of those initiated by the legislature. If we weight the difficulty of legislative initiative according to the number of states using each type of majority, we obtain a combined, weighted index score of 1.50 for legislative initiative, and thus an index score of 3.00 for popular initiatives (action 24).

Also, we know from state data going back to 1776 that the success rate of amendment proposals after popular referenda became the standard means of approval is virtually the same as when the agent of approval was the state legislature. We can thus say that a popular referendum used as a means for approving a proposed amendment (as opposed to initiating one) is about as difficult as having the state legislature approve it. We have just seen that the weighted average for bicameral legislative action is 1.50, and so we assign an index score of 1.50 to action 60, adding further increments for larger popular majorities, action 61-62. If we assume that special bodies act much the same as unicameral legislatures, the assigning of index scores for actions 2–3 and 32–38 is straightforward.

The use of American state data, in combination with straightforward assumptions, allows the generation of index scores for all actions except numbers 27–30 and 63–68. These actions are assigned index scores that seem reasonable in the context of the other index scores.

The index score assigned to the amendment process found in a national constitution is generated by adding together the numbers assigned by the index in Appendix B to every step required by that particular amendment process. Where a constitution provides for more than one path to a formal amendment, the score for each amendment path is weighted according to the percentage of amendments passed by means of it during the relevant time period.

How the index works can be illustrated by using it with the amendment process described in Article V of the U.S. Constitution. There is more than one path to amendment, and each must be evaluated. A two-thirds vote by Congress, since it requires two houses to initiate the process, is worth 1.60; whereas initiation by two-thirds of the state legislatures is worth 2.25. The latter path leads to a national convention, which uses majority rule in advancing a proposal,

TABLE 5

The Amendment Rate and Average Duration of Selected National Constitutions

	AMENDMENT RATE							
	.00–.24	.25–.74	.75–1.24	1.25–.74	1.75–2.24	2.25–.74	2.75–3.24	3.25+
Average Duration	43 (8)	50 (2)	96 (5)	47 (3)	19 (1)	17 (2)	26 (10)	

Note: Index scores are normed to that of stop #14, simple majority approval in a bicameral legislature, by dividing the approval rate for this baseline by the approval rate for each of the other possible actions. The increments are the estimated increase in the difficulty of passing amendments relative to the baseline rules.

thus adding .75 (under the assumption that this special body is elected). The first path still totals 1.60, and the other now totals 3.00. Ratification by three-fourths of the states through either their legislatures or elected conventions adds 3.50. The path beginning with Congress now totals 5.10, while the path beginning with the state legislatures and using a national convention totals 6.50. Even though the second path has never been successful (and one can see more clearly now why), it is still a valid option. For the total amendment process we can use the lower figure unless or until the more difficult procedure is ever used. That is, since the 6.50 path has never been used, a weighted composite score would be 5.10, which is what I shall use here. Appendix table C-1 shows the index of difficulty scores calculated for the national constitutions of 32 countries, along with other constitutional characteristics.

Performing the same calculation for the American states, I find that the average index score is 2.92, with very little variance. The highest state score is 3.60 (Delaware), and 26 states are tied for the lowest score at 2.75. Another 16 states have a score of 3.10. Thus, while one can detect variance between select subsets of states, the range of variance in general is very small compared with that found in the constitutions of other nations.

We have reached a point where we can now begin to test our propositions using data from the constitutions of other nations.

CROSS-NATIONAL AMENDMENT PATTERNS

Comparative cross-national data show that the U.S. Constitution has the second-most-difficult amendment process. This implies, if propositions 2 and 4 are correct, that the amendment rate for the Constitution may be too low, because its amendment procedure is too difficult, while the average amendment rate for the state constitutions is not too high.

There is an even stronger relationship between the length of a constitution and its amendment rate here than I found with the American state constitutions, with a correlation coefficient of .7970 (versus .6249 for the states) significant at the .0001 level.

The curvilinear relationship found between the amendment rate and average duration of American state constitutions is almost duplicated here in shape, strength, and high point. For the national constitutions, <#> is .75–1.24 (# = .95), and the high point is 96 years in average duration (see Table 5), while for the states, # is .75–1.00 (# = .89), and the high point is 100 years in average duration.[3] Both sets of constitutions studied have a similar moderate range of amendment rate that tends to be associated with constitutional longevity.

There is enough variance in the index of difficulty among cross-national constitutions for us now to test proposition 2 with some degree of confidence. Figure 1 illustrates that there is a very strong relationship (significant at the .001 level) between the index of difficulty and the amendment rate.[4] The more difficult the amendment process, the lower the amendment rate, and vice versa.

FIGURE 1

Cross-National Pattern for Amendment Rate and Difficulty, Indicating Amendment Strategy

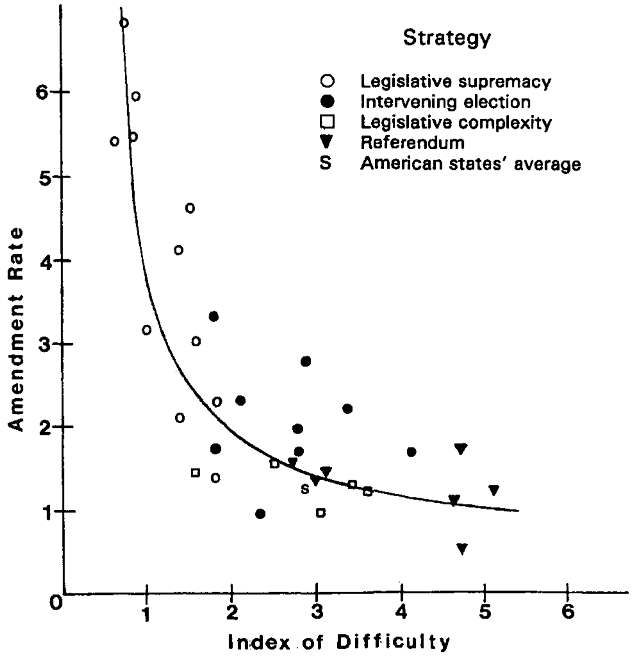

TABLE 6

Comparison of National Constitutions Grouped According to Their General Amendment Strategy

	AMENDMENT STRATEGIES			
	LEGISLATIVE SUPREMACY	INTERVENING ELECTION (DOUBLE VOTE)	LEGISLATIVE COMPLEXITY (REFERENDUM THREAT)	REQUIRED REFERENDUM OR EQUIVALENT
	Austria Botswana Brazil Germany India Kenya Malaysia New Zealand Papua N.G. Portugal Samoa	Argentina Belgium Colombia Costa Rica Finland Greece Iceland Luxembourg Norway	Chile France Italy Spain Sweden	Australia Denmark Ireland Japan Switzerland U.S. Venezuela
Average				
Index score	1.23	2.39	2.79	4.01
Length	59,400	13,000	18,300	11,200
Amendment rate	5.60	1.30	1.19	.28

That the relationship between amendment rate and difficulty of amendment process is highly curvilinear is more interesting than if it were simply a linear one, since there is a relatively small part of the curve where most of the effect is concentrated. This confirms the existence of a range of amendment rates that is more critical and toward which one should aim if constitutional stability is being sought. But it also suggests that one can with some confidence achieve a moderate rate of amendment by selecting an appropriate range of amendment difficulty. This, in turn, suggests that certain amendment strategies are better in this regard than others—a topic to which I now turn.

The difficulty of the amendment process chosen by framers of constitutions seems related to the framers' relative commitment to the premises used by the Americans when they invented the formal amendment process: (1) popular sovereignty, (2) a deliberative process, and (3) the distinction between normal legislation and constitutional matters. We can use these assumptions to group our 32 national constitutions into one of four general amendment strategies.[5]

Strategy 1 can be labelled *legislative supremacy* (Table 6, col. 1). Constitutions in this category reflect the unbridled dominance of the legislature by making one legislative vote sufficient to amend the constitution. The data reveal that the size of the majority required for this vote does not affect the amendment rate. The legislative supremacy strategy reflects a minimal commitment to the three American premises just listed. Strategy 2 is to require that the national legislature approve an amendment by votes in two sessions with an *intervening election* (col. 2). The national legislature is still basically in control, but the amendment process is made more deliberative, a clearer distinction is drawn between normal legislation and constitutional matters, and the people have an opportunity to influence the process during the election, which implies a stronger commitment to popular sovereignty. Sometimes other requirements diminish legislative dominance; the introduction of a nonlegislative body in the process (e.g., a constitutional commission) is typical. The double vote with an intervening election is the key change, so strategy (2) is termed the Intervening Election Strategy. As we move from strategy 1 to strategy 2 the amendment rate falls by 77%. Approximately half (54%) of this drop is explained by the 78% reduction in the average length of a constitution, and about half is explained by the 94% increase in the index of difficulty that results primarily from the double-vote, intervening election strategy.

Strategy 3 relies on *legislative complexity* (col. 3), usually characterized by multiple paths for the amending process, which features the possibility of a referendum as a kind of threat to bypass the legislature. A referendum can usually be called by a small legislative minority, the executive, or an initiative from a small percentage of the electorate—and often any of the three. This complexity and easy availability of a referendum emphasizes even more strongly the deliberative process, the distinction between constitutional and normal legislative, and popular sovereignty.

The legislative complexity strategy produces only an 8% reduction in the amendment rate compared with the intervening election strategy, although this is a 31% improvement over what we would expect, given the increased average length of strategy 3 documents. The slight overall improvement is due to the modest (18%) increase in the difficulty of the strategy. Strategies 2 and 3 together show most clearly how one can achieve similar amendment rates by trading off between constitutional length and amendment difficulty.

Strategy 4 institutionalizes the most direct form of popular sovereignty and also emphasizes to the greatest extent both the deliberative process and the distinction between constitutional and normal legislative matters. These countries have a *required referendum* as the final part of the process. The United States is placed in this category because the various appeals to the citizenry required by both amendment paths approximates a referendum and because the United States does not approximate any other strategy even remotely.

Compared to strategy 3, the referendum strategy's rate of amendment falls off 76%. This reduction to what is barely one-twentieth the rate for legislative-supremacy countries is about evenly explained by the 44% increase in difficulty vis-à-vis strategy 3 and a 39% reduction in average length.

Table 6 also shows that countries that use a referendum strategy, as well as those that use an intervening election strategy, have, on average, much shorter constitutions than the countries using the other strategies. They tend to have framework constitutions that define the basic institutions and the decision-making process connecting these institutions. The nations using strategy 1 tend to use a code-of-law form of constitution containing many details about preferred policy outcomes. These constitutions tend to be much longer. A code-of-law form of constitution implies a reduced distinction between normal legislation and constitutional matters vis-à-vis framework constitutions since normal legislation is put into the document. The code-of-law form, long documents, an easy amendment process, and legislative supremacy are all characteristics of the parliamentary sovereignty model that dominates the list of countries using strategy 1. New Zealand has perhaps the purest parliamentary sovereignty government in the world, and Kenya, India, Malaysia, Papua New Guinea, Botswana, and Western Samoa are not far behind. Although the countries using strategy 2 still have a fairly low index of difficulty, their much shorter constitutions indicate that a much greater divide has been crossed with respect to strategy 1 countries than the addition of a double vote with intervening election might imply.

Table 6 implies several interesting things that require emphasis. First, one can trade off between shorter length and greater difficulty to produce a similar amendment rate or use them together to produce a desired amendment rate. One can relax the level of difficulty and greatly reduce the rate of amendment simply by shortening a constitution. Second, it was determined earlier that the amendment rate is highly correlated with the degree of difficulty. It is now apparent that different amendment strategies, which reflect different combinations of assumptions about constitutionalism, have certain levels of difficulty associated with them. That is, institutions have definite (and in this case predictable) consequences for the political process.

Figure 1 demonstrates this rather clearly. The 11 nations that use the legislative supremacy strategy are grouped toward the low difficulty–high amendment rate end of the curve. These are left as an open *o*. The seven nations that use a referendum strategy, each indicated by an inverted triangle, are clustered toward the other end of the curve. Those that use legislative complexity are indicated by an open square, and those that use an intervening election strategy are represented by a filled-in *o*. The clustering of the various countries by amendment strategy shows that the averages reported in Table 6 represent real tendencies and are not merely statistical artifacts produced by averaging. The average for the American states is shown by an *s*.

Figure 1 shows the relationship between difficulty of amendment and amendment rate while controlling for the effects of length, which has the effect of shifting the curve upward a bit from that created by using raw index scores. The shifted curve is almost hyperbolic, which means that the relationship between difficulty of amendment and amendment rate can be approximated by the equation for a hyperbolic curve, $x = 1/y$.

An analysis of American state constitutions, with the difficulty of amendment held roughly constant by the similarity in their formal processes, reveals a relationship between the length of a constitution and its amendment rate that is described by a linear curve of best fit with a slope of .60—which is to say that on average, for every additional 10 thousand words, the amendment rate goes up by six-tenths of an amendment per year. The curve of best fit for the national constitutions, when controlling for the difficulty of the amendment process and when excluding the extreme cases of New Zealand and Japan, has a slope of .59. Those writing a new constitution can expect with some confidence, therefore, that there will be about a .60 increase in the amendment rate for every 10-thousand-word increase in the length of the document.

Finally, we might conclude from Table 6 that both the length of a constitution and the difficulty of amendment may be related to the relative presence of an attitude that views the constitution as a higher law rather than as a receptacle for normal legislation. Certainly it seems to be the case that a low amendment rate can either reflect a reliance on judicial revision or else encourage such reliance in the face of needed change. It is possible that the great difficulty faced in amending the U.S. Constitution led to heavy judicial interpretation as a virtue in the face of necessity.

The theory of constitutional amendment advanced here has posited a connection between the four methods of constitutional modification. Propositions 1–4 developed the concept of amendment rate in such a way that we were able to show an empirical relationship between the formal amendment of a constitution and its complete replacement. Propositions 5–8 used amendment rate to relate these two methods of modification to judicial and legislative alteration.[6] At this point I can now systematically include these last two methods in the overall theory. Toward that end, it is worth reconsidering briefly propositions 5–8 in the light of my findings on the amendment process in national constitutions.

PROPOSITION 5. *A low amendment rate, associated with a long average constitutional duration, strongly implies the use of some alternate means of revision to supplement the formal amendment process.*

The countries that have an amendment rate below <#> (defined as .75–1.24 for national constitutions) and also have a constitution older than the international average of 51 years include Australia, Finland, Ireland, and the United States. The proposition implies that these four countries either have found an alternative means (judicial review in the United States) or are under strong pressure to find another means. Denmark, Germany, Iceland, Italy, and Japan are all within a few years of falling into the same category, and if the proposition is at all useful, they should experience progressively stronger inclinations toward either a more active judiciary or a new constitution in the coming decades. A trend toward an active judiciary is already well advanced in Germany and is also becoming apparent in Japan.

PROPOSITION 6. *In the absence of a high rate of constitutional replacement, the lower the rate of formal amendment the more likely the process of revision is dominated by a judicial body.*

In the absence of further research, there is only indirect evidence for this proposition. Table 6 shows that the lower the rate of amendment, the less the legislature dominates. The executive is usually not a major actor in a formal amendment process, so we are left with the judiciary.

PROPOSITION 7. *The higher the formal amendment rate, a) the less likely the constitution is being viewed as a higher law, b) the less likely a distinction is being drawn between constitutional matters and normal legislation, c) the more likely the constitution is being viewed as a code, and d) the more likely the formal amendment process is dominated by the legislature.*

Discussion of Table 6 has supported all parts of this proposition.

PROPOSITION 8. *The more important the role of the judiciary in constitutional modification, the less likely the judiciary is to use a theory of strict construction.*

In the absence of further research, proposition 8 is a prediction to be tested.

CONCLUSION

I have examined two sets of constitutions. Each set is composed of documents that are taken seriously as constitutions. Every document in these two sets has a formal amendment process that is self-sufficient; that is, it depends on no other constitution to carry out a formal amendment of itself. The two sets of constitutions examined together comprise at least three-fourths of the existing documents defined by these two characteristics.[7]

A comparative, empirical study of the amendment process in these 82 documents leads to four specific conclusions about the amendment process, as well as four more general conclusions about constitutions. The first specific conclusion is that the variance in amendment rate is largely explained by the interaction of two variables: the length of the constitution and the difficulty of the amendment process.

Second, it is possible to manipulate these two variables to produce more or less predictable rates of amendment. The strong linear effects of length and the hyperbolic curve that describes the effects of difficulty—these two together allow us to formulate an equation that generates a pattern of amendment rates close to what we found empirically. If we let A represent the amendment rate, D the score on the index of difficulty, and L the length of a constitution in words, the equation representing their interrelationship is

$$A = [1/D + ((L/10{,}000) \times .6)] - .3.^{8}$$

Third, there is evidence that the amendment rate affects the probability that a constitution will be replaced and that a moderate amendment rate (between .75 and 1.25 amendments per year) is conducive to constitutional longevity.

Fourth, beyond a certain point, making the amendment process more difficult is an "inefficient" way to keep the amendment rate in the moderate range. Rather, it is easiest to do so by avoiding the extremes of either the legislative dominance or the referendum strategies and combining either the legislative complexity or the intervening election strategy with a relatively short document (10–20 thousand words).

Among more general conclusions, the first is that institutions have consequences and that the effects of institutional definitions in constitutions can be studied empirically.

Second, the similarity in amendment patterns between the American state constitutions and the national constitutions raises the possibility that for other aspects of constitutional design, one set of documents may be useful in developing propositions for studying the other set and therefore that there are basic principles of constitutional design operating independently of cultural, historical, geographic, and short-term political considerations.

Third, the first two general conclusions together suggest the possibility of discovering a set of principles that can be used to design constitutions with predictable results.

Fourth, the study of the amendment process strongly suggests that constitutional institutions cannot be studied in isolation from each other. Just as the operation of the legislature may strongly affect the patterns we find in the amendment process, the design of the amendment process may affect the operation of the court; and seemingly unrelated aspects of a constitution (e.g., its length and formal amendment process) may be linked in their consequences.

Finally, it is interesting that Buchanan and Tulloch's rational cost analysis receives some empirical support, although with a twist. Their general principle holds that constitutional choice rests on a tradeoff between decision costs and external costs (Buchanan and Tulloch 1965). Since constitutions contain important political settlements, any amendment carries with it the danger of serious externalities. Although an apparently rational actor might seek a very difficult amendment process in order to minimize externalities, such a one might also attempt to minimize externalities by constitutionalizing their interests, since the more specific the policy content of the constitution on a topic, the less danger there is that unwanted externalities will be imposed. This latter analysis would imply a fairly easy amendment process. However, a process that allows one actor to safeguard its interests allows all actors to do so. The data in this study indicate that the more policy content there is in a constitution, the longer it becomes. Both an easy amendment process (which leads to greater length and thus a higher amendment rate) and a very difficult amendment process (which leads to a very low amendment rate) produce a higher probability that a constitution will be replaced entirely. Thus, the two short-range types of behavior likely to be engaged in by a rational actor are irrational in the long-run, because when a constitution is replaced, everything is once again up for grabs—a situation in which constitutional safeguards against external costs are no longer in effect at the very time externalities are threatened on all serious political matters. Therefore, a truly rational actor would seem to be one who attempts to avoid constitutional replacement and instead avoids an amendment rate that is too high or too low. This would seem to argue for constitutional brevity and a moderately difficult amendment process on grounds of rationality.

APPENDIX A

Table A-1 presents an index to measure the degree of difficulty associated with each amendment process.

APPENDIX B

Table B-1 lists the numbers assigned by the index to every step required by the amendment processes.

APPENDIX C

Table C-1 shows the index of difficulty scores tabulated for the national constitutions of 32 countries along with other constitutional characteristics.

TABLE A-1

Basic Data on American Constitutions, 1991

STATE	NO. OF CONSTITUTIONS	AVG. DURATION	CURRENT CONSTITUTION SINCE	YRS. IN EFFECT	ORIGINAL LENGTH IN WORDS	TIMES AMENDED	AMENDMENT RATE
Alabama	6	29	1901	90	65,400	726	8.07
Alaska	1	35	1959	32	11,800	22	.69
Arizona	1	80	1912	79	28,900	109	1.38
Arkansas	5	31	1874	117	24,100	76	.65
California	2	72	1879	112	21,400	471	4.21
Colorado	1	115	1876	115	22,000	115	1.00
Connecticut	4	54	1965	26	8,800	25	.96
Delaware	3	72	1897	94	19,000	119	1.27
Florida	6	25	1969	22	18,900	53	2.41
Georgia	10	21	1983	8	26,000	24	3.00
Hawaii	1	41	1959	32	16,800	82	2.56
Idaho	1	102	1890	101	18,800	107	1.06
Illinois	4	43	1971	20	12,900	6	.30
Indiana	2	88	1851	140	9,100	38	.27
Iowa	2	73	1857	134	9,700	48	.36
Kansas	1	132	1861	130	10,200	87	.67
Kentucky	4	50	1891	100	21,800	29	.29
Louisiana	11	16	1975	16	47,300	27	1.69
Maine	1	172	1820	171	10,100	157	.92
Maryland	4	54	1867	124	25,200	200	1.61
Massachusetts	1	211	1780	211	11,600	116	.55
Michigan	4	39	1964	27	18,600	16	.59
Minnesota	1	134	1858	133	8,500	112	.84
Mississippi	4	44	1890	101	20,100	102	1.01
Missouri	4	43	1945	46	39,300	74	1.61
Montana	2	51	1973	18	11,600	15	.83
Nebraska	2	63	1875	116	16,100	189	1.63
Nevada	1	127	1864	127	14,100	108	.85
New Hampshire	2	108	1784	207	8,000	142	.69
New Jersey	3	72	1948	43	16,400	39	.91
New Mexico	1	79	1912	79	22,000	120	1.52
New York	4	54	1895	96	26,800	207	2.16
North Carolina	3	72	1971	20	10,300	27	1.35
North Dakota	1	102	1889	102	18,100	125	1.23
Ohio	2	95	1851	140	14,200	145	1.04
Oklahoma	1	84	1907	84	58,200	133	1.58
Oregon	1	132	1859	132	11,200	188	1.42
Pennsylvania	5	43	1968	23	20,800	19	.83
Rhode Island	2	108	1843	148	7,400	53	.36
South Carolina	7	31	1896	95	21,900	463	4.87
South Dakota	1	102	1889	102	21,300	97	.95
Tennessee	3	65	1870	121	11,100	32	.26
Texas	5	29	1876	115	28,600	326	2.83
Utah	1	95	1896	95	13,900	77	.81
Vermont	3	71	1793	198	5,200	50	.25
Virginia	6	36	1971	20	18,100	20	1.00
Washington	1	102	1889	102	16,300	86	.84
West Virginia	2	64	1872	119	15,900	62	.52
Wisconsin	1	143	1848	143	11,400	124	.87
Wyoming	1	101	1890	101	20,800	57	.56
Mean	2.9	77	1896	95	19,300	117	1.23
U.S. Constitution		202	1789	202	4,300	26	.13

Sources: The data in this appendix are based on James Q. Dealey, *Growth of American State Constitutions* (New York: Da Capo, 1972); Walter F. Dodd, *The Revision and Amendment of State Constitutions*, 2d ed. (New York: Da Capo, 1970); Daniel J. Elazar, *American Federalism: A View from the States*, 2d ed. (New York: Thomas Y. Crowell, 1972); Fletcher M. Green, *Constitutional Development in the South Atlantic States, 1776–1860* (New York: Da Capo, 1971); Ellis Paxson Oberholzer, *The Referendum in America* (New York: Da Capo, 1971); Harold W. Stanley and Richard G. Niemi, *Vital Statistics on American Politics* (Washington: Congressional Quarterly, 1992); Albert L. Sturm, *Thirty Years of State Constitution-Making: 1938–1968* (New York: National Municipal League, 1970).

TABLE B-1

An Index for Estimating the Relative Difficulty of an Amendment Process

ACTION	CONSTITUTIONAL REQUIREMENT	ADD
	Initiation Requires Action by	
1	An executive	.25
2	A special appointed body	.50
3	A special elected body	.75
	A unicameral legislature	
	Legislative approval	
4	By a majority of 1/2 + 1	.50
5	*Twice* using 1/2 + 1	.50
6	By an absolute majority[a]	.65
7	*Twice* by absolute majority	.65
8	By a 3/5 majority	.65
9	*Twice* by 3/5 majority	.65
10	By a 2/3 majority	.80
11	By a 3/4 majority	.90
12	*Twice* by a 2/3 majority	1.75
13	If an election is required between two votes	.25
	Action by a bicameral legislature	
	Legislative approval	
14	By a majority of 1/2 + 1	1.00
15	*Twice* using 1/2 + 1	1.00
16	By an absolute majority	1.25
17	*Twice* by absolute majority	1.25
18	By a 3/5 majority	1.25
19	*Twice* by a 3/5 majority	1.25
20	By a 2/3 majority	1.60
21	By a 3/4 majority	1.80
22	*Twice* by a 2/3 majority	3.55
23	If an election is required between two votes	.50
	A petition	
24	Of 0–250,000 voters	3.00
25	By 250,000–500,000 voters	3.50
26	By more than 500,000 voters	4.00
	Multiple state	
27	Legislatures, 1/2 + 1	2.00
28	Conventions, 1/2 + 1	2.00
29	Legislatures or conventions, 2/3	3.00
30	Legislatures or conventions, 3/4	3.50
	Approval Requires	
31	Action by an executive	.50
	Approval by a special body	
32	1/3 or less	.25
33	1/2 + 1	.50
34	Absolute majority	.65
35	3/5 majority	.65
36	2/3 majority	.80
37	3/4 majority	.90
38	3/4 majority	.90
	If any of the above acts a second time	.50
	Action by a unicameral legislature	
	Legislative approval	
39	1/3 majority or less	.25
40	1/2 + 1	.50
41	*Twice* by 1/2 + 1	.50
42	Absolute majority	.65
43	*Twice* by absolute majority	.65
44	3/5 majority	.65
45	*Twice* by 3/5 majority	.65
46	2/3 majority	.80
47	3/4 majority	.90
48	If an election is required between two votes	.25
49	Legislative approval *twice* by 2/3 majority	1.75
	Action by a bicameral legislature	
	Legislative approval	
50	1/3 majority or less	.50
51	1/2 + 1	1.00
52	Absolute majority	1.25
53	*Twice* by absolute majority	1.25
54	3/5 majority	1.25
55	*Twice* by 3/5 majority	1.25
56	2/3 majority	1.60
57	3/4 majority	1.80
58	*Twice* by 2/3 majority	3.55
59	If an election is required between two votes	.50
	A popular referendum	
60	1/2 + 1	1.50
61	Absolute majority	1.75
62	3/5 or more	2.00
	Multiple state	
63	Legislatures, 1/2 + 1	2.00
64	Coventions, 1/2 + 1	2.00
65	Legislatures or conventions, 2/3	3.00
66	Legislatures or conventions, 3/4	3.50
67	Majority of voters *and* majority of states	3.75
68	Unanimous approval by state governments	4.00

[a] "Absolute majority" is used to indicate a requirement for approval by 1/2 + 1 of the *entire body*, whereas "1/2 + 1" indicates a requirement for approval by 1/2 + 1 of those voting.

Notes

Research for this article was supported by a grant from the Earhart Foundation of Ann Arbor, Michigan. Assistance in data analysis was provided by Patricia Gail McVey of the University of Texas School of Public Health.

1. While this was the first explicit amendment process in a state constitution, a formal amendment process was first used in William Penn's 1678 *Frame of Government*, which may explain why Pennsylvania was the first state to adopt one (see Viel 1992, 11–12).

2. These data can be found in Sturm 1982, 90.

3. The test for curvilinearity using cross-national data is neither strictly comparable to that used for the American states nor an adequate test of the relationship using national constitutions. Whereas the entire constitutional history for all 50 American states was used, only the most recent period of constitutional stability exceeding 15 years was used for the cross-national data. The arbitrary use of a 15-year minimum may well exaggerate the average longevity of national constitutions, and the use of only the most recent minimum period may weaken the results.

4. Figure 1 was generated using the statistical package

TABLE C-1

Basic Data on Selected National Constitutions

COUNTRY	AMENDMENT RATE	INDEX OF DIFFICULTY	AMENDED LENGTH IN WORDS	YRS.	TIME PERIOD
Argentina	1.04	2.10	10,600	87	1853–1940
Australia	.09	4.65	11,500	91	1901–92
Austria	6.30	.80	36,000	17	1975–92
Belgium	2.30	2.85	10,700	15	1973–88
Botswana	2.44	1.30	35,600	18	1966–84
Brazil	6.28	1.55	58,400	18	1969–87
Chile	.64	3.05	24,200	45	1925–70
Columbia	1.73	2.75	25,100	95	1886–1981
Costa Rica	1.26	4.10	15,100	33	1949–82
Denmark	.17	2.75	6,000	39	1953–92
Finland	.86	2.30	18,300	73	1919–92
France	.19	2.50	6,500	24	1968–92
Germany	2.91	1.60	22,400	43	1949–92
Greece	1.32	1.80	22,100	17	1975–92
Iceland	.21	2.75	3,800	48	1944–92
India	7.29	1.81	95,000	42	1950–92
Ireland	.55	3.00	16,000	55	1937–92
Italy	.24	3.40	11,300	46	1946–92
Kenya	3.28	1.00	31,500	18	1964–81
Japan	0.00	3.10	5,400	47	1945–92
Luxembourg	1.80	1.80	4,700	19	1968–87
Malaysia	5.18	1.60	91,400	35	1957–92
New Zealand	13.42	.50	180,000	40	1947–87
Norway	1.14	3.35	6,500	178	1814–1982
Papua New Guinea	6.90	.77	53,700	17	1975–92
Portugal	6.67	.80	26,700	15	1976–91
Spain	.18	3.60	8,700	24	1968–92
Sweden	4.72	1.40	40,800	18	1974–92
Switzerland	.78	4.75	13,300	119	1873–1992
United States	.13	5.10	7,400	203	1789–1992
Venezuela	.24	4.75	20,500	25	1967–92
Western Samoa	.95	1.80	22,500	22	1962–84
Average	2.54	2.50	29,400	52	

Note: Cross-national constitutional data have been taken from the constitutions themselves and from commentaries on these documents, found primarily in Albert P. Blaustein and Gisbert H. Flanz, *Constitutions of the Countries of the World*, 19 vols. (Dobbs Ferry, NY: Oceana, 1987) and supplements.

STATA, and the figure was then reproduced by hand using Geotype overlay techniques to enhance readability.

5. These broad categories were constructed using the theoretical premises developed herein and thus are independent of any categorization schemes developed previously by others. For an instructive comparison, see Lijphart 1984, 189–91. In order for a country to be included, it had to have at least one 15-year period free of military rule or serious instability, during which constitutionalism was taken seriously. Reliable data on the number and nature of amendments for that country also had to be available to the researcher. The unavailability of such data explain the absence of the Netherlands, for example, or for Austria before 1975.

6. On this topic, see Antieu 1982 and McWhinney 1956.

7. Canadian provincial and Australian state constitutions are prominent among those remaining to be examined. Also, Israel, Canada, and the United Kingdom, although lacking a simple written constitution, remain to be included. The problem in each of these three cases lies in determining what has constitutional status. An initial attempt to do so, using the content of the New Zealand Constitution as a template, yielded the following very preliminary estimates for two of these legislative supremacy countries:

	Israel	*United Kingdom*
Amendment rate	2.5+	7.5+
Index of difficulty	.50	.60
Length	10,000+	250,000+
Relevant years	1949–91	1900–1991

8. One part of the equation factors the effects of length in by dividing the number of words in the constitution by 10,000 and multiplying by .60. The second part approximates the effects of amendment difficulty by using the formula for a hyperbolic curve: A = 1/D. However, this is only approximate, and subtracting .30 from the effects of amendment difficulty results in the curve of best fit for the raw data scores.

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American Political Science Review Vol. 88, No. 2 June 1994

EXPECTATIONS AND PREFERENCES IN BRITISH GENERAL ELECTIONS

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We address two questions: How do people form their expectations about the likely winner of the next general election? and What are the links between expectations and votes? Using data collected by the Gallup organization in Great Britain, we find that the expectations formation process (1) has a significant inertia component but also a rapid adjustment to current information; (2) reflects voters' ability to translate economic expectations into political forecasts; and (3) is "time-bounded," possessing special characteristics immediately before and after a general election. The analysis also confirms the existence of a small bandwagon effect, whereby expectations that one party will win inflate that party's vote. The ability of voters to make reasonable forecasts without being unduly influenced by their own preferences suggests that under normal circumstances voters are expressing real preferences and not simply following the crowd.

The idea that observed contemporary behaviors are strongly linked to expectations about future outcomes has become so central for economists that much of economic theory has been rewritten to take explicit account of this concept (Begg 1982; Hudson 1982, chap. 3). Political scientists have also shown a growing interest in expectations, with future prospects proving to be particularly useful in studying certain aspects of electoral behavior. The now-common use of the notions of viability and electability, as well as the classification of voters as straightforward, sincere, or strategic, exemplify this trend (Abramson et al. 1992; Bartels 1988; Niemi, Whitten, and Franklin 1992). The simple act of voting is increasingly conceived of in terms of utilities (voters' preferences) and probabilities (voters' assessments of candidates' chances of winning).

Two crucial questions are at the nexus of the votes–expectations literature: How do people form their expectations and What are the links between expectations and votes? We address these questions with data collected by the Gallup organization. This unique data set includes not only the usual measurement of voters' preferences among parties but also a systematic tracking of voters' expectations about the outcome of the next general election.[1] The fact that this information is available on a monthly basis for more than a decade permits systematic study of the expectations formation process and of the links between expectations and votes not only during election periods but also in less intense, interelection times.[2] The data are thus attractive despite their obvious limitations.[3]

We shall present a simultaneous-equation votes–expectations model,[4] paying particular attention to the expectations formation process.[5] We then present our findings, which show a modest effect of expectations on voting behavior and stronger and more variegated effects—including "instantaneous" adjustments—of voting intentions on expectations. We discuss the theoretical implications of our findings in the conclusion.

A VOTES–EXPECTATIONS MODEL

The votes–expectations literature has been mainly concerned with the existence and form of a pattern of reciprocal causation between voters' preferences and their assessments of the electoral campaigns (i.e., the parties' standings). Consistent with this perspective, we examine here two possibilities: (1) that voting intentions are affected by expectations about the likely winner of the next general election (bandwagon effect) and (2) that expectations about which party will win the next election are dependent on current vote intentions (whether through a sophisticated treatment of information or a simple projection effect).

Practically speaking, what is required is that we devise and estimate a simultaneous equations system formed of a vote equation and an expectations equation. As is standard in simultaneous equations models, we estimate our model using two-stage least squares. To obtain consistent estimates, we must pay particular attention to model specification and estimation, especially the less-frequently-studied expectations equation. Substantive questions concerning the appropriate specification of the model will be discussed. Identification issues, estimation strategy, and diagnostic tests are described in Appendices A and B.

The Vote Equation

To specify the vote equation of our simultaneous-equation model, we build from the extensive literature on the short-term dynamics of political party popularity in Britain (Alt 1979; Clarke, Mishler, and Whiteley 1990; Clarke, Stewart, and Zuk 1986; Dunleavey and Husbands 1985; Frey and Schneider 1978;

Goodhart and Bhansali 1970; Hibbs 1982; Hudson 1985; Lewis-Beck 1988; Mishler, Hoskin, and Fitzgerald 1989; Miller and Mackie 1973; Norpoth 1991; Pissarides 1980; Sanders, Ward, and Marsh 1991). In particular, we searched for a satisfactory mix of economic and political indicators to serve as control variables whose inclusion would prevent us from inflating the impact of the expectations term. The specification we arrived at follows closely the prescription of Clarke, Mishler, and Whiteley:

> A properly specified model of government popularity requires four broad classes of explanatory variables: . . . the long-term predispositions of voters, . . . issue perceptions which include retrospective and prospective thinking about economic and noneconomic issues, . . . leadership images, which relate both to the government and opposition parties, . . . [and] various systematic and random shocks to the system. (1990, 66)

Our specification is shown in equation 1:

$$\begin{aligned} \mathrm{VCL}_t = {} & \beta_0 + \beta_1 \mathrm{VCL}_{t-1} + \beta_2 \mathrm{MPSHARE}_t + \beta_3 \mathrm{UNEMP}_{t-1} \\ & + \beta_4 \mathrm{INF}_{t-1} + \beta_5 \mathrm{PEXP}_t + \beta_6 \mathrm{LEADERS}_t \\ & + \beta_7 \mathrm{FALK} + \beta_8 \mathrm{WCL}_t, \end{aligned} \tag{1}$$

where VCL is the percent intending to vote Conservative minus percent intending to vote Labour (percent calculated after excluding *don't knows*, *undecideds*, *refusals*); MPSHARE is the ratio of the percent intending to vote for a minor party to the sum of the percents intending to vote Conservative or Labour; UNEMP is the unemployment rate, monthly, seasonally adjusted; INF is the monthly inflation rate calculated from the adjusted retail price index; PEXP is the percent thinking that personal economic conditions will improve over the next 12 months minus percent thinking that personal economic conditions will worsen over the next 12 months; LEADERS is the percent approval of the prime minister minus percent approval of the opposition leader. FALK is a dummy variable equal to 1 for May–June 1982 (the Falklands War), 0 otherwise; and WCL is the percent thinking that the Conservatives are most likely to win the next general election minus percent thinking that Labour is most likely to win.[6]

The coding of the variables and the form of the equation deserve brief explanation. Our major concern, of course, is with the impact of expectations about the winner—WCL.[7] It is assumed that this impact is instantaneous, that is, that the gap measured in the current poll is already reflected in vote intentions. The assumption of immediate effects will be discussed in considerable detail, when we make a similar assumption about the effects of vote intentions on expectations. The expectations variable itself is expressed as a percentage difference because the popularity gap between the parties is at the core of thinking of one as a potential winner and the other as a loser. Likewise, the vote intentions variable is measured as a percentage difference.

The most interesting of the remaining variables in the vote equation is VCL_{t-1}. Under normal circumstances vote intentions "do not move abruptly from one moment to the next one, but at a sluggish pace instead" (Norpoth 1991, 146). We include the lagged dependent variable to capture this inertia component. Following Norpoth, this variable may be conceived as reflecting a "short-term memory" process, whereas our constant term may be an indicator of the "long-term component of popularity" (pp. 146–47).

Based on Mishler, Hoskin, and Fitzgerald's (1989) results, we also checked, by including the MPSHARE variable, whether the party balance between Conservatives and Labour was affected by the electoral fortunes of third parties. As theory and recent empirical work suggest that voters' evaluations of government economic performance may be a blend of both objective conditions and subjective perceptions,[8] we added to our vote equation the unemployment rate, the inflation rate, and a variable reflecting voters' prospective assessments of their personal economic situations over the next 12 months.[9] The inclusion of unemployment and inflation appears sensible because both are "highlighted by the media, and salient to the voter" (Clarke, Mishler, and Whiteley 1990, 66; see also Mosley 1984).[10] The subjective perception variable (PEXP) was selected after various tries with similar variables.[11]

Inclusion of the variable LEADERS follows what has become standard practice in British studies. Given the finding by Mishler, Hoskin, and Fitzgerald that "the relationship between leader satisfaction and party support is highly significant" (1989, 228), it is also consistent with our goal of not inflating the expectations term in our vote equation. Moreover, this inclusion also seems justified on theoretical grounds, as "recent research suggests that although related, leadership satisfaction and party popularity are conceptually distinct and empirically separable (Clarke, Mishler, and Whiteley 1990, 68).[12] Finally, the Falklands War exemplifies a major disruption to the usual pattern of stability, and a variable was included to take it into account.[13]

The Expectations Equation

In the search for an appropriate specification for our expectations equation, we faced more difficulties than for the vote equation. First, while popularity functions have long been available and have been extensively studied, such is not the case for expectations; there is no extensive body of research on expectations comparable to that for vote intentions. Second, our dependent variable—the expectations question—lacks the immediate time horizon of the vote question (who will win "the next general election" versus one's vote if there were a general election "tomorrow").

Some insight can be derived from work on expectations in economics. Early attempts to model the expectation formation process centered on the "adaptive" expectations model, where lagged values of a variable were used to predict current inflation or other relevant economic quantities (e.g., Cagan 1956 and Nerlove 1958; a good summary is Nerlove, Grether, and Carvalho 1979, chap. 10). These initial attempts were subsequently criticized as being "atheoretical" and inconsistent with any explicit optimizing behavior on the part of individuals within the model. Later research in economics argued that expectations were formed "rationally" or in a "model consistent" manner (Lucas 1973; Nerlove, Grether, and Carvalho 1979, chap. 10; Sargent and Wallace 1975); that is, expectations of an economic variable should be based upon all the variables in the model and not just the past values of that variable.[14]

Our approach to modeling the expectations formation process lies somewhere between these two polar cases. Like the early critics of the "adaptive" expectations process, we recognize that more than lagged values of a variable are required to explain how individuals make forecasts of quantities they find important. However, we also recognize that the rational expectations model used in economics may be too strong for our explicitly political analysis. In particular, an empirical economic model that incorporates the rational expectations assumption will typically include a series of economic variables in the expectations equation. This is because economic agents are assumed to use these variables in forecasting, say, inflation or unemployment. Given the time horizon of the economists' expectations model, this strategy makes perfect sense. One is typically interested in forecasting inflation or any other economic variable for the *next* period.

In our political model the argument is different. The agents in our model are voters, and they are often asked to forecast an event (the election outcome) many periods from now. Consequently, economic information in the form of inflation and unemployment rates will not be particularly useful since current values provide very little predictive power for economic variables possibly two or three years from today.[15] Other restrictions to the expectations equation appear plausible as well. The leader variable used in the vote equation is strongly retrospective rather than prospective,[16] and the minor party share variable certainly does not capture the complexities related to the translation of small parties' short-term success into major parties' future electoral prospects. Finally, we theorize that single events such as the Falklands War are unlikely to influence expectations except insofar as they leave their imprint on the parties' standings.

The variables that will be important in forming our "political" expectations are those with an avowedly political component, including such factors as parties' levels of support, party reputation, and the period in the electoral cycle, plus one economic variable that asks respondents to look forward a full year. The following equation reflects this mode of thinking:[17]

$$\mathrm{WCL}_t = \alpha_0 + \alpha_1\mathrm{WCL}_{t-1} + \alpha_2\textsc{postelect} * \mathrm{WCL}_{t-1} + \alpha_3\mathrm{VCL}_t + \alpha_4\textsc{preelect} * \mathrm{VCL}_t + \alpha_5\textsc{pexp}_t + \alpha_6\mathrm{D8387},$$

where WCL, VCL, and PEXP are defined as in equation 1; POSTELECT is a dummy variable equal to 1 in May–July 1979 and July–September 1983, 0 otherwise; PREELECT is a dummy variable equal to 1 in February–April 1979 and April–June 1983 and 1987, 0 otherwise; and D8387 is a dummy variable equal to 1 from July 1983 to June 1987, 0 otherwise.

The equation suggests, first of all, that there is an inertia component to expectations about the winner of the next election. Some individuals—through general lack of attention to politics, a failure during specific periods to absorb new information about the parties, or a heavy discounting of new information for partisan or other reasons—fail to reassess their expectations (or do so only after some delay). The lagged dependent variable taps this inertia.

In addition, one might include an inertia component in the expectations formula as used here because of the form of the measurement available to us. Our dependent variable reflects the aggregation of individual responses to a question allowing only nominal choices of the most likely winning party. Yet we would hypothesize that individuals form their expectations on a continuous scale (e.g., from 0 to 100). Thus, when asked about the most likely winner of the next election, one might considerably reassess the subjective probability that a party will defeat its opponents without crossing the 50% threshold. Such individuals, constrained by the format of the question in the Gallup surveys, will appear unchanged even though they have thoroughly reassessed a party's chances. Thus, in addition to capturing inattentiveness to political events, the lagged term will reflect hypothesized fine changes that are masked by the available measurement.

The second term in the expectations equation reflects the fact that electoral expectations—including lagged effects—are not "time-free." They are, on the contrary, "time-bounded" by the event about which they are formed, namely, the next election (see, e.g., Lewis-Beck and Skalaban 1989, 150–51). One aspect of this timing is that voters adopt a different perspective in forming their expectations in the months immediately after an election. To illustrate this point, consider the 1983 election. In the last Gallup poll available, fully 87% of the respondents believed that the Tories would win the election, and only 5% thought Labour would win. (WCL, our dependent variable, reaches its highest point, 82%, at that time.) In the month immediately following the election, the same percentages were, respectively, 49 and 16. After an election, voters clearly keep in mind that a new ball game has begun. To represent this discontinuity in expectations after the election, we included an interactive term, POSTELECT * WCL_{t-1}, in our model.[18]

Of greatest interest in the expectations equation are the next two variables, which together constitute an adjustment for *current* voting intentions. Theoretically, we are adopting here the portion of the rational expectations view that says that individuals "use current available and relevant information in forming their expectations and do not rely purely upon past experiences" (Shaw 1983, 47). Specifically, we are assuming that *current* information about vote intentions affects perceptions about which party is likely to win the next election.[19] Of course, in a literal sense individuals cannot be reacting to the results of a survey that is only then being conducted. But people do have very recent, even simultaneous, bases for their answers.

Information about candidates, parties, and politics bombards voters on a daily basis. Some of it comes directly in the form of news reports from the media; some of it is filtered through friends and acquaintances. Some of it is directly in the form of horse-race reports;[20] some of it is in the form of stories about candidates' characteristics and activities. Any single story, of course, may reach only a small minority of the population, and only a fraction of those may be persuaded to update their expectations about which party is likely to win the next election. Thus, as indicated, there is a degree of inertia in people's expectations. But along with this inertia, very recent and contemporary events and reports are indeed likely to be incorporated into current expectations.[21]

In addition, individuals may reason that their current evaluations of the parties are likely to be shared by others. For example, a Labour identifier dissatisfied with his party's leaders and policies to the point of expressing a preference for the Tories may quite reasonably believe that other Labourites feel the same way (and that Conservatives, on the other hand, feel good about their party) and—as a consequence—feel that his party's electoral prospects are quite poor. The correspondence over time of voting intentions and expectations may thus be the result of a quite sophisticated treatment of political information.

Of course, simple projection may occur as well, as voters unconsciously align their expectations with their preferences. To this degree, also, expectations will mirror the current proportions intending to vote for each party. Unfortunately, it may be impossible ever to separate these processes completely, even with individual-level data. We can show quite convincingly, however, that what we are tapping goes well beyond that kind of projection.[22]

A hypothesis of simple projection would make no allowance for variations between election and interelection periods. But there are, as noted, strong theoretical reasons to believe that the process of forming expectations will have a different character in the neighborhood of the forthcoming election. First, as election day approaches, information about the parties' standings becomes a cheap and easily available commodity. Second, the incentive to use this information increases, as some voters might want to engage in tactical voting. If the degree of connection represents more than mindless projection, the electoral period should be characterized by a more pronounced *dis*connection between preferences and expectations as voters adjust their views about party's winning chances more quickly and more sensitively to relevant political information than in more quiet, nonelection times.

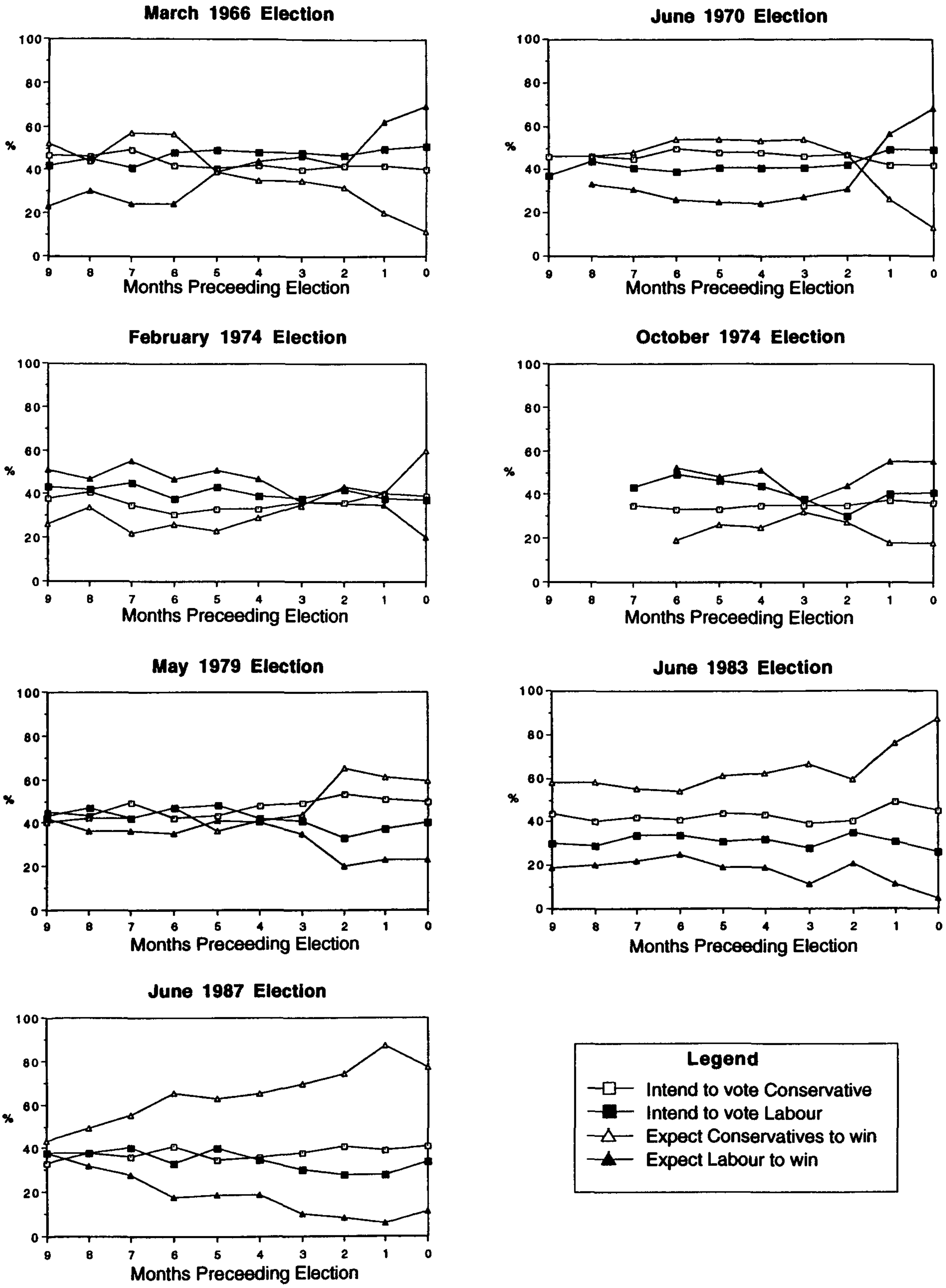
That wishful thinking is harder to maintain as the election approaches can be seen in Figure 1, which show the relationship between voting intentions and expectations during the nine months prior to each of the general elections in Britain between 1966 and 1987.[23] In every case, one finds the expected disconnection between votes and expectations, as even loyal partisans are forced to admit that they will lose their electoral bid.[24] The form, duration, and "spread" of the party's chances of winning vary from one election to the other, but the graphs seem to show a consistent pattern, one that would not be evident if projection was the only force at work. To take this pattern into account, we have included an interactive term, PREELECT $*$ VCL_t,[25] to capture the disconnection between preferences and expectations during the electoral period.[26]

This disconnection should manifest itself in a positive coefficient for the PREELECT $*$ VCL_t variable. To understand this, consider an example in which the Conservatives have the support of 50% of the electorate and Labour the support of 40% during the interelection period (VCL = 10) and in which there is a great deal of wishful thinking. Conservative and Labour supporters alike may suppose their party will win, suggesting a coefficient of approximately 1 between vote intentions and expectations of which party will win, so that net of other influences, WCL should be approximately 10 as well. Now suppose that as the election approaches, there is no change in vote intentions: it is still the case that the Conservatives have the support of 50% of the electorate and Labour the support of 40%. Those who support the Conservatives have no incentive to change their minds about which party should win the election; all 50% should still expect a Conservative victory. However, the connection between vote intentions and expectations should weaken for Labour supporters. Some of the 40%, despite their preference, will concede that the Conservatives are likely to win. This weakening of the connection between preferences and expectations means that a given vote-intention difference between the Conservatives and Labour (VCL remains 10) should translate into a larger proportion of individuals expecting the advantaged party to win (WCL becomes greater than 10); that is, the coefficient of the interaction term should be positive.[27]

The two remaining variables in the expectations model have similarities to adjustment and inertia components, respectively. First, we include prospective economic thinking because the link between the state of the economy and party popularity has been demonstrated repeatedly by scholars in any number of countries. And inasmuch as these judgments are about the next 12 months, they are less subject to the problems we pointed to earlier in justifying the *exclusion* of unemployment and inflation variables. In addition, forecasting on the basis of future personal economic situations makes sense both in terms of retrospective and prospective thinking. If one forecasts outside the electoral period, the performance of the economy over the next 12 months is relevant

FIGURE 1

Voting Intentions and Election Forecasting During Three Quarters Prior to British General Elections, 1966–1987



information, as it will influence the judgments of retrospective thinkers in a forthcoming election. In electoral periods as well, prospective perceptions are relevant as one may hypothesize that other voters evaluate the performance of the incumbent government in terms of the future.[28]

Finally, what might be regarded as another form of inertia would manifest itself when a political party was able to establish a reputation as a winner. This reputation can develop from the subtle interplay of consecutive victories,[29] the opposition's weakness,[30] leadership strength,[31] and past accomplishments.[32] These requirements seem to have been met during the second mandate of Margaret Thatcher as voters perceived her party as the likely winner of the next election even in light of poor showings in contemporary polls. This winner's reputation component is tapped in our model by a dummy variable equal to 1 for this period.[33] As this variable intends to capture a phenomenon that is clearly specific to the expectations formation process, namely the rigidity of expectations under particular circumstances, it is not included in the vote equation. This does not mean that rigidity is without consequences on the parties' standing, as it will have an impact through the WCL variable in the vote equation.

RESULTS

Tables 1 and 2 show our main results.[34] With respect to vote intentions (Table 1), the first point is that the mix of economic and political variables in equation 1, along with the lagged dependent variable, appear to form an appropriate group of control variables to assess the feedback effect of expectations on voting intentions. Our strategy was to devise a vote equation that would prevent us from inflating the impact of the expectations variable, and we seem to have accomplished that goal. First, the tests reported in Appendix B for possible misspecification are reassuring on this point. Second, our results are sufficiently in accord with theoretical expectations and with other empirical results to suggest that the more standard portion of the equation yields a reasonable account of vote intentions sans expectations.

As in most models of party support, for example, a lagged or autoregressive effect is clearly significant and sizable (e.g., Mishler, Hoskin, and Fitzgerald 1989).[35] That some—but not necessarily all—economic factors are significant is a common result (Clarke, Mishler, and Whiteley 1990, 69–80; Mishler, Hoskin, Fitzgerald 1989, 231; Norpoth 1991, 151–52). Likewise, other studies have found only small effects of minor-party variables, whereas comparative leadership evaluations have been found to be substantial (Clarke, Mishler, and Whiteley 1990, 71; Mishler, Hoskin, and Fitzgerald 1989, 231). There are some differences between our results and those of other studies, but they are most likely attributable to different definitions of the sample period or to differences in variable definitions.[36]

TABLE 1

Voting Intentions in Britain as a Function of Economic, Leadership, System Factors, and Vote Expectations

VARIABLE	COEFFICIENT
Intercept	3.69** (1.25)
Vote intention (VCL) at time t − 1[a]	.21** (.06)
Minor party share at time t[b]	−.06 (.06)
Unemployment rate at time t − 1[c]	−.33** (.09)
Inflation rate at time t − 1[d]	.08 (.42)
Personal economic expectations at time t[e]	.10** (.04)
Leadership approval at time t[f]	.27** (.03)
Falklands War period[g]	4.64* (2.73)
Forecast of expected winner (WCL_t)[h]	.10** (.02)

Note: R^2 = .91, N = 138. Entries are unstandardized regression coefficients; standard errors are in parentheses. Dependent variable (VCL_t) is vote intention calculated after excluding *don't knows*, *undecideds*, and "refusals"). See also n. 10.

[a] % intending to vote Cons. − % intending Lab.

[b] % minor party/(% Cons. + % Lab.).

[c] Monthly, adjusted seasonally.

[d] Monthly rate, retail price index.

[e] % expecting better − % expecting worse.

[f] % approval of PM − % approval of opp. leader.

[g] 1 for May–June 1982, 0 otherwise.

[h] Instrumental variable. Instrument is the predicted value generated by regressing WCL on all exogenous variables in the system; WCL is defined in Table 2. See Appendix A for details.

*$p < .05$.

**$p < .01$ (one-tailed test).

Assuming, then, that our model is a reasonable one, we find a relatively small but clearly statistically significant effect of expectations on voting intentions. Every 10-point gap between the percentages thinking that the Conservatives rather than Labour will win the next election contributes one point to the electoral margin itself. Since 20-to-30-point gaps are not infrequent even in the interelection periods (see Figure 1) and since the margin of vote intentions is often fairly small, such an impact is far from negligible. Of course, one should not overestimate the bandwagon effect either. Even in the weeks immediately prior to the election, when the expectation gap may reach as high as 50–60%, the *change* in WCL from the interelection period may only account for an additional two or three percentage points. Moreover, as we shall shortly note, expectations themselves are highly dependent on vote intentions.[37] Nonetheless, it is clear that the bandwagon effect, while relatively small in

TABLE 2

Forecasts of the Expected Winner of the Next General Election in Britain as a Function of Economic Expectations, Vote Intentions, and Electoral Periods

VARIABLE	COEFFICIENT
Intercept	.78
	(1.16)
Forecast of expected winner (WCL) at time t − 1[a]	.54**
	(.05)
Postelection * expected winner forecast (t − 1)[b]	−.30**
	(.08)
Vote intention (VCL$_t$)[c]	1.33**
	(.18)
Preelection * vote intention (VCL$_t$)[d]	.92**
	(.24)
Personal economic expectations at time t[e]	.21**
	(.09)
Conservative winning reputation[f]	9.39**
	(1.94)

Note: $R^2 = .91$, N = 138. Entries are unstandardized regression coefficients; standard errors are in parentheses. Dependent variable (WCL$_t$) is forecast of expected winner.

[a] % expecting Cons. to win − % expecting Lab. to win.

[b] Postelection = 1 for three months, 0 otherwise.

[c] Instrumental variable. Instrument is the predicted value generated by regressing VCL on all exogenous variables in the system; VCL is defined in Table 1. See Appendix A for details.

[d] Preelection = 1 for three months, 0 otherwise.

[e] % expecting better − % expecting worse.

[f] 1 for July 1983–June 1987, 0 otherwise.

*$p < .05$.

**$p < .01$ (one-tailed test).

magnitude, may be of no small consequence in a closely contested election.

Turning to the expectations equation, we noted earlier that there was less certainty about how to formulate it properly. As a consequence, all of the findings are of interest. Overall, the results (Table 2) indicate that the expectations formation process (1) exhibits both significant inertia and adjustment components, (2) reflects the voters' ability to translate expected economic conditions into political forecasts, and (3) is "time-bounded," possessing special effects both before and after the electoral period.

First, note the strength of the inertia effects. To a greater extent than vote intentions, expectations about which party will win depend on prior expectations (i.e., the lagged dependent variable). An exception is in the months immediately following an election, in which the negative coefficient for the interactive term, POSTELECT * WCL$_{t-1}$, means that voters recognize that the next election is a long way off and that it may therefore have little in common with what has just transpired. The consistent nine-point bulge toward the Conservatives during the 1983–87 period suggests that there may be some long periods in which one party has a kind of built-in edge. Though additional time and data are required before we can explore this effect more thoroughly, we suspect that it may take on different forms in different times and places.[38]

Next note that despite these multiple sources of inertia, winning expectations are also subject to several sources of adjustment. As anticipated, there is a strong connection between current vote intentions and expectations. A 10-point gap in the proportions presently intending to vote for the two parties typically translates into a more than 13-point difference in which party is expected to win. During the months immediately preceding an election, this effect is boosted, by the coefficient on the PREELECT * VCL$_t$ interaction term, to nearly a 25-point difference.

As noted, it is not possible completely to disentangle the degree to which the effect of VCL represents rational adjustment to contemporary information or blind projection. But the strength of the interaction term means that voters *are* able to disconnect themselves to a significant degree from their basic partisan loyalties when they perform political forecasts in the neighborhood of a forthcoming election. At least in the context of relative informational clarity of the immediate preelection period, losers as well as winners generally reach the same conclusion about the identity of the likely winner of the next election.

Finally, the significant coefficient for personal economic experiences, PEXP, indicates that election expectations, like vote intentions, are tied to projections of future economic conditions. In general, optimistic economic forecasts boost the incumbent's expected chances of reelection. While the precise form of this relationship may vary from time to time, the general message is that voters are able to link economic forecasts and expectations much as they link economic forecasts with their own vote intentions.

CONCLUSION

Heretofore, most evidence about the interplay between election preferences and expectations has been derived from the study of American primary elections. The emphasis has been on the effects of expectations on preferences, as well as on the role of week-to-week events and of publicity in the mass media (Abramson et al. 1992; Bartels 1987, 1988; Brady and Johnston 1987; Shanks et al. 1985). This emphasis makes considerable sense in the context of primary elections, in which the contests are between individuals, not parties, the candidates are often poorly known at the start, and elections are held on a rolling basis.

Though some of the same forces are obviously at work, one cannot readily generalize from these studies of American primaries to the nature of expectations in general elections. Therefore, we developed a model to fit the latter circumstances. In doing so, we placed greater emphasis on the effects of preferences on expectations and on the nature of the expectations-formation process in less intense, interelection times as well as during election periods.

Our findings show, first of all, that the expectations-formation process takes on a different character during interelection times. Similarities exist between the electoral period and more quiet times (inertia exists throughout, and the economic forecast and winning reputation variables remain unchanged) but there is a major disconnection between voter preferences and expectations in the neighborhood of an election. Particularly revealing is the preelection adjustment (PREELECT $*$ VCL_t). It may be tempting to interpret the quick adjustment component (VCL_t) simply as a manifestation of what Bartels called "the resilience of projection . . . even in situations where objective cues about the shape of political reality are quite powerful" (1987, 811). Indeed, the fact that only the intensity of the electoral campaign introduces a strong reality check lends credence to the idea that most of the time, most of the people think that future outcomes will move in the direction of their own preferences. But the larger, combined effect of aggregate preferences and simple projection (and whatever other psychological mechanisms link current preferences and expectations) in periods close to the election tells us that reality rapidly takes over when the election is actually in sight.[39]

Our findings also reveal the contemporaneous nature, even during interelection periods, of the effect of vote intentions on expectations and vice versa. Before they were even completed, much less publicized by the media, the outcomes of polls of vote intentions were already internalized by respondents and reflected in the degree to which they thought that one party held the advantage going into the next general election. Likewise, the anticipated election outcomes had already influenced, though to a smaller degree, those same vote intentions.

While the contemporaneous impact of vote intentions on expectations is consistent with the notion of a simple projection effect, it also suggests how readily available is information about party popularity and related matters. Despite the little attention that ordinary voters pay to political matters, the aggregate movements studied here appear to reflect an ongoing awareness of and reaction to the political world. More generally, the results are also consistent with the view that people accommodate political information quickly and efficiently.

In making much the same point, Lewis-Beck and Skalaban suggested that about all that voters needed to perform competent electoral forecasts was "regular membership of the [American] polity" (1989, 153). Our own work goes one step further by suggesting that voters use quite simple forecasting rules, weighting, when it is relevant, the burden of the past (the winning reputation component) with current facts and evaluations about who they support, what they expect their future financial situation to be, and when the next election will occur.

With respect to vote intentions, our finding of a bandwagon effect, even though small, adds support to the contention that some voters respond to the implicit bonus of being on the winning side even in high-salience general elections. Bandwagon effects have been found primarily in U.S. presidential primaries (Abramson et al. 1992, 64–65; Bartels 1988; Skalaban 1988, 136–50), but as noted, those elections represent a very different setting from most electoral situations. In British studies, evidence consistent with bandwagon effects has been found, but the absence of panel data made it impossible to distinguish with certainty between bandwagon and projection effects (McAllister and Studlar 1991, 736). And in a Canadian study, there was almost no evidence of bandwagon effects (Johnston et al. 1992). It may well be that their small size not only makes bandwagon effects difficult to detect but that they are sometimes swamped by other election factors. At any rate, the existence of a bandwagon effect in our study suggests that in the future, properly specified popularity functions should include an expectations term whenever possible.

Finally, the results of the overall model are refreshing from a normative point of view. They indicate that many among the "losers" are able to make reasonable forecasts without being in turn heavily influenced by this disappointing outcome. That so many losers recognize themselves as such and nevertheless stick to their preferences is refreshing in that it strongly suggests that under normal circumstances voters are expressing preferences and not simply following the crowd.

APPENDIX A: IDENTIFICATION AND ESTIMATION OF THE EXPECTATIONS AND VOTE MODELS

This appendix gives details of the identification and estimation of our equations.

Identification

Identification of our model is complicated by the fact that we have a system of equations that is nonlinear in the variables. In particular, the expectations equation contains a current endogenous variable on the right-hand side that enters in both an additive and an interactive fashion. It is well known that the usual necessary-and-sufficient rank condition for identification of linear structural equations needs to be amended in this context (Achen 1986, 141–44; Greenberg and Webster 1983, 230–33; Kelejian and Oates 1989, 304–19). Fortunately the required rank condition is a simple extension of the linear case.

In a general linear simultaneous-equations model with G endogenous variables, the rank condition requires that for each equation in the system rank $(A\,\Phi) = G - 1$ where $A = [\Gamma\ B]$ is a matrix comprised of the coefficients on the endogenous and exogenous variables in the system and Φ is a conformable matrix representing the exclusion restrictions present in the system. In the linear-in-the-variables case, we require that the endogenous variables enter the A matrix (through Γ) additively. This rules out models like ours where the A matrix contains an interactive term due to the specification of the expectation equation. However, a simple extension of the rule rank (A Φ) = G − 1 will cover our nonlinear-in-the-variables case. (For a general proof see Fisher 1966, chap. 5). Provided we include the nonlinear function of the endogenous variable as an additional column of A–Γ, we can continue to use the rule rank (A Φ) = G − 1 to check the identification status of the model. In more practical terms, provided we treat the interactive term in the expectations equation as an additional endogenous variable holding constant the number of rows of A = [Γ B], it is simple to check the identification status of our expectations and vote equations. The reader will find that both our equations are overidentified.

Estimation

The estimation of our model is a relatively straightforward application of two-stage least squares—an instrumental-variables method in which the "instrument" is simply the predicted value of the endogenous variable(s) from a reduced-form regression equation. As with the identification procedure, estimation is slightly more complicated when the model is nonlinear-in-the-variables. In the linear simultaneous equations model the instrument is formed by regressing each endogenous variable in the model on all exogenous variables in the system. In the nonlinear case the instrument is often formed by regressing each endogenous variable on all the exogenous variables plus a set of polynomials and interactive terms in the exogenous regressors (see, e.g., Achen 1986, 142–43). We computed the instrument by the standard method, eschewing the more complex nonlinear-in-the-variables reduced form (although we tried this specification and it made little difference in the results). A justification for our choice of procedure can be found in Kelejian and Oates 1989, 319–24. Provided the nonlinearity is not too severe (and our reduced-form estimates indicate that this is the case), the linear approximation in the instrument equation will not cause problems in formulating the instrument.

As noted, the actual estimation procedure is two-stage least squares, which is simply a particular form of instrumental variables. The asymptotic theory that underlies estimation and inference in linear and nonlinear expectations models can be found in Pagan 1984, 1986. Our model is a variant of Pagan's (1984) model 2 with some additional attention being paid to the nonlinearity. Standard asymptotic theory on the convergence of the normalized moment matrix of regressors and the cross-moment matrix of regressors and disturbances leads to consistency results in the location (slope) parameters. As is standard, the residuals are calculated from the original variables leading to consistency for the scale (variance/standard error) parameter. Proofs are straightforward and can be found in Pagan 1984, 227–29 (the linear model) and Pagan 1986, 523–30 (the nonlinear model). The diagnostics, which indicate that our specification is sound, can be found in Appendix B.

APPENDIX B: DIAGNOSTIC TESTING PROCEDURES

To check for general adequacy of our model specification we used the diagnostic testing procedures advocated by Spanos (1986) and applied in political science by Granato (1991). The PC-GIVE package provides a series of regression diagnostics designed to alert one to general model misspecification. The tests provided include RESET tests for functional misspecification, a Lagrange multiplier test for general patterns of serially correlated errors, a test for ARCH (Autoregressive Conditional Heteroscedasticity) patterns in the residuals, a test for general heteroscedasticity in the residuals (the "White" test), and a test for residual normality. Because we are operating in a simultaneous-equations framework, we use the residuals based on the original variables, not their predicted values. The relevant statistics based on these residuals converge to the chi-squared distribution, as do their single equation counterparts (Spanos 1986, chaps. 25–26). The substantive aspects of our regression diagnostics follow.

First, our model appears to be well specified: the RESET test for both vote and expectation equations indicates that there is no obvious misspecification in terms of functional form or omitted variables. The relevant statistics fall well below conventional levels of significance ($\chi_5^2 = 3.30$ for the expectations equation, and $\chi_4^2 = 1.53$ for the vote equation).

Second, serially correlated errors do not plague our analysis: given that our model includes a lagged dependent variable, the standard Durbin–Watson statistic is not very meaningful. A more appropriate and general test is the Lagrange multiplier test. Using PC-GIVE, we performed a joint test on lags 1–7 of the autocorrelation coefficient. The resulting statistics fall well below conventional levels of significance ($\chi_7^2 = 1.49$ for the expectations equation, and $\chi_7^2 = .22$ in the vote equation). Additional evidence confirming the absence of serial correlation can be seen by looking at the magnitude of the autocorrelation coefficients at lags 1–7. No estimated coefficient is larger than .20, and most of them are in the −.05 to .05 range.

There does appear to be extremely mild heteroscedasticity for the expectations equation ($\chi_{13}^2 = 22.96$, significant at the .05 but not at the .01 level) and no visible traces of it in the vote equation ($\chi_{16}^2 = 14.56$). The evidence on lack of heteroscedasticity, while not quite as good as that on autocorrelation, seems to be reasonable.

Finally, both the expectations and the vote equations have residuals that pass a χ^2 test for normality ($\chi_2^2 = 1.40$ for expectations and $\chi_2^2 = 5.61$ for vote).

In summary, model diagnostics indicate that our two-equation model is theoretically well specified, does not have serious problems in the stochastic component, and possesses residuals that are normally distributed.

Notes

We would like to thank Kirke Lawton for excellent technical assistance. We also wish to thank Neal Beck, Helmut Norpoth, and William Keech for their comments on a draft of this article.

1. The voting intention question is, "If there were a General Election tomorrow, how would you vote?" The expectations question is, "Regardless of how you are going to vote yourself, which party do you think is most likely to win the next general election?"

2. The expectations question was asked irregularly (mostly in months just preceding an election) from 1945 to 1973. Since 1975, it has been asked every month except seven months after the 1979 elections and five months after the 1987 elections. We limit our attention to the period starting in January 1976 and ending in June 1987, for two basic reasons: (1) including the period after the 1987 election would have been demanding in econometric terms as it would have created a second string of missing data; and (2) since the popularity function of the British parties has been extensively studied for different subperiods up to the end of the second mandate of Margaret Thatcher, it will be easier to specify a satisfactory vote equation, a preliminary condition in any study about the links between votes and expectations. On the popularity function, see Clarke, Mishler, and Whiteley 1990; Mishler, Hoskin, and Fitzgerald 1989; Norpoth 1991; Sanders, Ward, and Marsh 1991.

3. The original expressions of both preferences and expectations are categorical rather than thermometers measuring intensity of feelings and estimated probabilities. In addition, we have available only aggregate rather than individual-level preferences and expectations (though aggregate data may well be better than individual data, as noted in Kramer 1983 and Lewis-Beck and Skalaban 1989, 153).

4. We shall sometimes use the terms *votes* and *votes equation* rather than the more precise but longer terms *voting intentions* or *popularity function*. Similarly, we shall use *expectations about future elections* and *election forecasts* interchangeably.

5. We formulate the model in terms of general elections. So far, the most intense scrutiny of expectations formation has been in U.S. primary elections. We discuss the differences between those models and our own in the conclusion.

6. VCL, WCL, LEADERS, and MPSHARE are taken from Butler and Butler 1986 updated to 1987 from the *Gallup Poll Monthly*. UNEMP and INF are taken from the Organization for Economic Cooperation and Development's *Main Economic Indicators: Historical Statistics*. PEXP is taken from the *Gallup Poll Monthly*. As indicated in n. 2, WCL was unavailable for seven months during the period under study. We estimated our models both with and without fitted values (computed from the model's parameters) for the missing data and obtained nearly identical results in terms of the parameters and the goodness-of-fit measures. Results with fitted values are reported here.

7. For the purposes of estimation, the endogeneity of WCL_t and VCL_t poses a problem when they appear as explanatory variables in the vote and expectation equations, respectively. For this reason, an instrumental variable in the form of a predicted value is created when either variable appears on the right-hand side of our equations. See Appendix A for details.

8. Inclusion of both objective conditions and personal expectations has been common in the popularity function literature in Great Britain ("A Computer's Election Advice," *Economist*, 17 August 1991, 53; Clarke, Mishler, and Whiteley 1990; Sanders, Ward, and Marsh 1991). Some work on U.S. presidential popularity argues in favor of including only retrospective (Beck 1989) or only prospective (MacKuen, Erikson, and Stimson 1992) economic variables. Our interest here is not in testing these alternative views but to take reasonable precautions against overestimating the effects of expectations.

9. How do you think the financial situation of your household will change over the next 12 months? (*Get a lot better, Get a little better, Stay the same, Get a little worse, Get a lot worse*). Following Clarke, Mishler, and Whiteley 1990 and Sanders, Ward and Marsh 1991, we operationalized the variable as the percentage difference between the first two categories (*a lot better* or *a little better*) and the fourth and the fifth categories (*a little worse* or *a lot worse*).

10. Note also that both variables (as well as the subjective perception and the LEADERS variables) were designed in accordance with a reward–punishment model. To do so, "we reverse the sign of unemployment and inflation during Conservative governments thereby predicting that the Conservative . . . [lead] will increase both: a) as inflation and unemployment rise during Labour governments and b) as inflation and unemployment decline during Conservative governments" (Mishler, Hoskin, and Fitzgerald 1989, 224). Note finally that the rival hypothesis, the issue priority model, was also examined. (We also checked the possibility that the Tories may have benefitted from an "incumbency effect" by adding a dummy variable taking the value of 1 for the two Conservative mandates. It was not significant.) A useful summary of the differences between the issue priority model and the standard reward–punishment model is Clarke, Stewart, and Zuk 1986, 127–30. See also Kiewiet 1983; Kiewiet and Rivers 1984; Mishler, Hoskin, and Fitzgerald 1989, 222–24; Peffly 1984.

11. Retrospective perceptions about both personal and general economic conditions, as well as a variable measuring prospective perceptions about general economic conditions, were also examined.

12. Lafay, in a French study, follows a rather similar practice by introducing the popularity index of the prime minister in an equation for the popularity function of the president (1991, 135). Micro analyses also show that evaluations of party leader performance in Britain have independent effects on party support (see Clarke, Stewart, and Zuk 1986; Graetz and McAllister 1987). For evidence that leader images in Britain affect not only short-term intentions such as voting but also long-term intentions such as party identification, see Clarke and Stewart 1984.

13. There has been a debate about the magnitude and form of the impact of the Falklands War on parties' standings. Consequently, we tried various specifications for this variable and selected the best one using standard criteria. We think it likely that our simple dummy variable, together with the LEADERS variable, adequately takes into account the immediate Falklands War effect and its lasting impact through the sustained popularity of Mrs. Thatcher. On the debate over the war's effects, see Clarke, Stewart, and Zuk 1986; Clarke, Mishler, and Whiteley 1990; Norpoth 1987a and b; Sanders, Ward, and Marsh 1991.

14. Specifically, one would write down a structural model of the economy and use all the variables in the system to help predict the expected value of the endogenous variable of interest. Technically, this would impose a series of nonlinear cross-equation restrictions between the "reduced form" equation and the "expectations" equation. This is not the strategy we pursue here, for reasons outlined in the text.

15. This would be especially true if the data were "difference-stationary" rather than "trend-stationary." However, in our model only the inflation variables might reasonably be viewed as a difference-stationary process (but see Ostrom and Smith 1993).

16. First, the component *satisfaction with the prime minister* is basically retrospective. Second, the question about the opposition leader is also retrospective in that it does not ask the respondent to think about the opposition leader as a potential prime minister. Third, the two components do not tie the leaders together in an explicit inquiry about their comparative

potential as future prime minister. The question, "Who would make the best prime minister?," followed by a list of potential leaders, would perhaps be a better choice in an expectations equation, but it has not been asked on a regular basis. An example will illustrate these various points. In December 1985, satisfaction with Prime Minister Thatcher was 39%, with Kinnock at 47%; yet when asked who would make the best prime minister, Thatcher received the support of 33%, Kinnock of 24%.

17. Our expectations model is consistent with the argument outlined in Beck 1991. Beck argues that one ought to think of economic forecasts as conditional expectations where the conditioning variables include competence, administration, and other political variables. He goes on to criticize the strong rational-expectations approach in the popularity-function literature (e.g., Chappell and Keech 1985; Chrystal and Peel 1985) and presents empirical evidence indicating that such a model is suspect. (See also the criticisms of the rational-expectations approach in Hudson 1982, chap. 3.) We believe that our expectations model, which is more than an adaptive expectation but less than the economists' rational expectations, is an appropriate forecasting model.

18. This approach is similar to that used in studying presidential popularity functions, in which some account needs to be taken of changes in administrations. See, e.g., Beck 1991, 91, n. 11).

19. The high correlation between current intentions and expectations (.80 for the period under study) supports the view that individuals update their views about the likely winner of the next election very quickly, even continuously.

20. Evidence about the availability of poll results can be found in Butler and Kavanagh 1987. Evidence about public exposure to polls for the elections under study here can be found in McAllister and Studlar 1991, 725.

21. Indeed, to *exclude* VCL_t from the equation would require, instead, that we include variables representing sources of all the new information used to form the voter's current vote intention. It is likely, of course, that some voters are sufficiently insulated from contemporary information that they seldom update their understanding of voting intentions. Our argument is that on balance, the most immediate measure of vote intentions (i.e., at time t) is a superior predictor of current expectations. We tested this reasoning, however, by trying an alternative specification in which VCL_{t-1} replaced VCL_t. The resulting coefficient was not significant despite the high correlation (.78) between the two vote measures.

22. In fact, if simple projection from one's own vote intentions were the only process involved, then only individual-level VCL would be appropriate in the expectations equation. However, our interpretation is much broader. As we noted, individuals probably feel that their current evaluations are shared by others, but surely they do not think that everyone else views the parties identically. Thus, it seems fully justifiable to include aggregate-level VCL in the expectations equation. The interaction of VCL with PREELECT is also a strong indication that aggregate VCL belongs in the expectations model.

23. We expand our time frame here because data for nine months prior to each election are available for the longer period.

24. Note that voters put great reliance on polls to form their expectations even if they were misleading, as in 1970 and 1974.

25. Do the additive variable PREELECT and the variable POSTELECT (introduced earlier) need to be included as well for proper specification? Not only is it unnecessary always to include each of the components of an interactive term (see, e.g., Lewis-Beck 1980, 54–56), but it is relatively meaningless in this case, as the correlation between PREELECT and PREELECT * VCL is .93, and that between POSTELECT and POSTELECT * WCL is .63. As to the choice of which variables to include (additive or interactive), theory clearly suggests the interactive form. If two parties are perceived as having exactly the same support, there is no reason to believe that the type of "blips" illustrated in Figure 1 will occur (as a dummy variable would suggest). Moreover, a dummy variable would imply that these blips will have the same magnitude whatever the vote difference between the main contenders. An interaction term is more flexible, as it allows the sudden increase of "nonprojecting" voters to be linked to the closeness of the election.

26. Throughout, the election period is operationalized as the two months preceding an election as well as the electoral month itself (with a slight adjustment in 1979, when the election was on 3 May). Though electoral campaigns as such are shorter than that in Great Britain, rumors and polling activities justify this definition. The graphs show that the election effect does not occur smoothly over the interelection period but rather abruptly in the neighborhood of a forthcoming election—presumably under pressure of the unmistakable realities of the electoral campaign, the enhanced attentiveness of the public, and the decreased likelihood that time will permit a trailing party to rebound significantly. The uniqueness of the electoral context also explains the abruptness of the postelection adjustments we have discussed.

27. That a comparable interaction term would be negative in a model of individual vote decisions is entirely consistent with our argument, as it is precisely the weakening of projection effects (during election periods) among those who are likely losers that will generate the widening of the gap between preferences and expectations illustrated in Figure 1 and in the example in the text.

28. We checked for a possible interaction between time (operationalized both as *one year before the election or not* and *number of months until the next election*) and PEXP and found it not significant; perhaps the effect of this variable is about the same in election and nonelection periods precisely because it can be relevant for both retrospective and prospective reasons.

29. The two consecutive victories by Margaret Thatcher followed a period during which the incumbent party had been defeated four times out of six (1964, 1970, 1974, and 1979). Moreover, her 1983 victory was the most impressive one in the postwar period both in terms of seats and the popularity gap with Labour. Conversely, Labour's performance in this election was the poorest one in seats and popular support for the same period.

30. Labour was perceived during this period as much more divided than the Tories. In fact, in all the surveys during Thatcher's second mandate, the percentage of respondents perceiving Labour as a divided party outnumbered by extremely large margins the percentage of those thinking of it as united (−55 on average). The Tories were basically perceived as a united party for the same period, with exceptional and short periods of negative balance. (see *Gallup Poll Monthly*).

31. The high personal popularity of Thatcher, as well as the Tory's advantage in questions about the best leaders, speak to this point (see *Gallup Poll Monthly*).

32. The victorious Falklands War, the decrease of the inflation rate, and the economic recovery are possible candidates here.

33. That we found out-of-sample confirmatory evidence of the rigidity of expectations during Thatcher's third mandate buttresses our confidence that the type of inertia measured by our winning reputation variable is a genuine characteristic of expectations. Moreover, a preliminary inspection of the data after the Conservatives' fourth consecutive victory in 1992 apparently shows less rigidity, which is also consistent with our hypothesis that a string of victories is a necessary—though not a sufficient—condition to produce rigidity in expectations.

34. R^2 is estimated as 1–Regression Sum of Squares/Total Sum of Squares.

35. Should we formally test for integration of order one (I(1)) or a "stochastic nonstationarity" in the data series? We feel that such integrated behavior is highly unlikely, given the departure of our lagged coefficients from unity and the fact that our data are rates or ratio-type variables. For a discussion of this point and some general problems with the unit root methodology involved, see Cochrane 1991, 207–9.

36. The reported effect of the Falklands variable may seem small compared to other work. However, it is important not to lose sight of two facts. First, the results will be different depending on the definition of the dependent variable. The war had a different impact on Thatcher's approval rate (immediate and lasting) from what it did on the Tories' popularity (immediate but rapidly declining). Second, the results will be greater if the approval rate of Thatcher is excluded from the regressors rather than included, as here.

37. In addition, we tested for the possibility that bandwagon effects were greater in the period immediately prior to elections (by including an interactive term, PREELECT * WCL). The coefficient was not significant. Following Norpoth, we also checked the possibility that electoral campaigns drive disenchanted partisans back to the governing party (1991, 150–51); we did so by adding separate dummies for the electoral periods under study. None of these variables was significant.

38. For example, in a Canadian study, a simple counting variable denoting the number of consecutive polls in which a given party had led in preelection polls was important. We tried such a variable here, but it was not significant (perhaps because of the much greater time between polls, which meant that winning streaks were probably remembered less well, and, in any event, were not so long lived (in terms of numbers of polls) as those in Canada. We also tried a variable measuring the proportion of times during the last six months in which the Conservatives were leading in the polls, but it was not significant (see Johnston et al. 1992, chap. 7).

39. Sometimes, reality becomes overwhelming, and "almost everyone" correctly forecasts the election result, as in Britain in 1983 (see Figure 1) or in the U.S. elections of 1964 and 1972 (Lewis-Beck and Skalaban 1989, 148). Of course, even in the case of a landslide, there will be some "true believers." Moreover, a more adequate measure of expectations (0–100 scale) would probably capture projection effects not visible with the measure we have available; for example, in the U.S. presidential election of 1964, a staunch Republican might have given Goldwater a 25% chance of winning, while most others would have given him close to a 0% chance.

40. These authors also point out that British work on this topic has produced conflicting results; see McAllister and Studlar 1991, 724. That work has often been based on simple comparisons between election results and public opinion polls conducted during the campaign.

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American Political Science Review Vol. 88, No. 2 June 1994

COLLECTIVE IDENTITY FORMATION AND THE INTERNATIONAL STATE

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The neorealist–neoliberal debate about the possibilities for collective action in international relations has been based on a shared commitment to Mancur Olson's rationalist definition of the problem as one of getting exogenously given egoists to cooperate. Treating this assumption as a de facto hypothesis about world politics, I articulate the rival claim that interaction at the systemic level changes state identities and interests. The causes of state egoism do not justify always treating it as given. Insights from critical international relations and integration theories suggest how collective identity among states could emerge endogenously at the systemic level. Such a process would generate cooperation that neither neorealists nor neoliberals expect and help transform systemic anarchy into an "international state"—a transnational structure of political authority that might undermine territorial democracy. I show how broadening systemic theory beyond rationalist concerns can help it to explain structural change in world politics.

The collective action problem dominates world politics. The state is often thought to solve it in domestic society by forcing or socializing people to identify with the common good, but the problem seems endemic to the anarchic world of international relations, where each state reserves the right and the force to do as it pleases. Realists and idealists have long debated the conditions under which states can overcome this problem, the former taking a materialist line that power and human nature preclude significant cooperation, the latter arguing that knowledge and institutions make it possible. The discussion has become very sophisticated, with neorealists and neoliberals today dissecting the effects of pursuing relative versus absolute gains, the shadow of the future, transaction costs, and so on (Baldwin 1993).

An important but neglected feature of the recent debate is the shared belief that collective action should be analyzed in terms of Mancur Olson's (1965) definition of the problem, which takes self-interested actors as constant and exogenously given and focuses on the selective incentives that might induce them to cooperate. This reflects the rationalist strategy of explaining behavior in terms of changing prices or constraints, rather than tastes (Stigler and Becker 1977). While some neoliberals have questioned the assumption of state egoism (Keohane 1984, 120–32; see also Lumsdaine 1993), few treat state interests as *endogenous* to interaction.[1] They either bracket the formation of interests, treating them *as if* they were exogenous, or explain interests by reference to domestic politics, on the assumption that they are exogenous, although not necessarily constant (e.g., Moravcsik 1992). In both cases, the effect on systemic theory is captured by what Jeffrey Legro (1993) calls the rationalist "two-step": first interests are formed outside the interaction context, and then the latter is treated as though it only affected behavior. This can be merely a methodological presumption, but given its pervasiveness in the current debate it may also be seen as an implicit hypothesis about world politics: systemic interaction does not transform state interests.

Neoliberals make things hard for themselves by accepting this constraint, and their efforts to explain cooperation under it are admirable. But it also brackets an important line of argument against realists, namely, that through interaction, states might form collective identities and interests, redefining the terms of Olson's problem altogether (Calhoun 1991). We cannot know, a priori, which argument is more appropriate. A rationalist approach makes sense when state interests really are exogenous to interaction, which is sometimes the case. When they are not, however, it may ignore important possibilities and/or strategies for cooperation, as well as misrepresent the latter's dynamics.

It would be useful to discuss potential anomalies for the rationalist hypothesis, but ultimately it can only be assessed against its rival, which has not been adequately articulated in the literature. With a view toward theoretical pluralism, my goal herein is to formulate such a rival by reframing the collective action problem among states in terms that make interests endogenous to (or part of the problem in) interaction. In so doing, I hope to put in sharper relief the underappreciated implications of a rationalist assumption (exogeneity).

I draw on two literatures for this purpose. The first is integration theory, which focuses on the formation of community at the international level. Moribund since the early 1970s, it is undergoing a revival today, thanks in part to neoliberals (Keohane and Hoffmann 1991). Collective identification is an essential variable in this theory, however, since without changes in identity the most we can expect is behavioral cooperation, not community (Deutsch et al. 1957; Lasswell 1972; Russett 1963). A rejuvenated "sociology of international community" will therefore ultimately have to go beyond a rationalist vocabulary (Linklater 1990). This exists in what is variously called critical, reflectivist, or *constructivist* international relations scholarship (see Keohane 1988; Wendt 1992). Constructivists are interested in the construction of identities and interests and, as such, take a more sociological than economic approach to systemic theory. On this basis, they have argued that states are not structurally or exogenously given but constructed by historically contingent interactions.[2]

In building a bridge between these two literatures I take a state-centric approach, which integration theorists have often avoided in the past because of its emphasis on anarchy. I do so for two reasons. First, notwithstanding the growing importance of nonstate actors in world politics, states remain jealous of their sovereignty and so may resist collective identification more than other actors, which poses a harder case for theory. Second, I argue that collective identification is an important condition for the emergence of "international states," which would constitute a structural transformation of the Westphalian states system. In effect, constructivism shows how the concern of integrationist theorists with the formation of community can be addressed from a state-centric perspective and the latter thereby made into a critical theory of world politics.

I shall define my dependent variable—forms of identity and interest—then briefly discuss some causes of state egoism at various levels of analysis, arguing that none justifies treating it as given. I shall then sketch a "pretheory" of collective identity formation among states and, finally, suggest how this points to an internationalization of the state, with implications for the states system and democratic theory.

IDENTITY AND COLLECTIVE ACTION

Constructivism is a structural theory of the international system that makes the following core claims: (1) states are the principal units of analysis for international political theory; (2) the key structures in the states system are intersubjective, rather than material; and (3) state identities and interests are in important part constructed by these social structures, rather than given exogenously to the system by human nature or domestic politics. The second claim opposes realism. The third opposes systemic theories that are rationalist in form, whether they are "as if" theories that bracket interest formation, or unit-level, "reductionist" ones (Waltz 1979) that say interests "really are" exogenous. The result is one form of structural idealism or "idea-ism".[3]

The claim that states are socially constructed can take various forms. In an effort to avoid an oversocialized approach, I distinguish between the corporate and social constitution of state actors, which parallels the distinction between the "I" and the "me" in symbolic interactionism (Mead 1934). In both cases, I argue that interests are dependent on identities and so are not competing causal mechanisms but distinct phenomena—in the one case, motivational, in the other, cognitive and structural—and, as such, play different roles in explaining action.

Corporate identity refers to the intrinsic, self-organizing qualities that constitute actor individuality. For human beings, this means the body and experience of consciousness (Schwalbe 1991); for organizations, it means their constituent individuals, physical resources, and the shared beliefs and institutions in virtue of which individuals function as a "we" (Douglas 1986). Corporate identities have histories, but these do not concern me here; a theory of the states system need no more explain the existence of states than one of society need explain that of people. The result is a weak or essentialist social constructionism, but one that still leaves the *terms* of state individuality open to negotiation. (There is no space to defend it here, but for a start, see Leplin 1988 and Wendt n.d.)

The corporate identity of the state generates four basic interests or appetites:

1. physical security, including its differentiation from other actors
2. ontological security or predictability in relationships to the world, which creates a desire for stable social identities
3. recognition as an actor by others, above and beyond survival through brute force
4. development, in the sense of meeting the human aspiration for a better life, for which states are repositories at the collective level.[4]

These corporate interests provide motivational energy for engaging in action at all and, to that extent, are prior to interaction, but they do not entail self-interest in my sense, which is an inherently social phenomenon. How a state satisfies its corporate interests depends on how it defines the self in relation to the other, which is a function of social identities at both domestic and systemic levels of analysis.

Social identities are sets of meanings that an actor attributes to itself while taking the perspective of others, that is, as a social object (McCall and Simmons 1978, 61–100). In contrast to the singular quality of corporate identity, actors normally have multiple social identities that vary in salience. Also in contrast, social identities have both individual and social structural properties, being at once cognitive schemas that enable an actor to determine "who I am/we are" in a situation and positions in a social role structure of shared understandings and expectations. In this respect, they are a key link in the mutual constitution of agent and structure (Wendt 1987), embodying the terms of individuality through which agents relate to each other. These terms lead actors to see situations as calling for taking certain actions and thus for defining their interests in certain ways.

Some state identities and interests stem primarily from relations to domestic society ("liberal," "democratic"), others from international society ("hegemon," "balancer"). Foreign-policy role theorists (e.g. S. Walker 1987), as well as more recently a number of neoliberals, have emphasized the domestic (and thus systemically exogenous) roots of state identities. I am interested in showing how state identities may be endogenous to the system but (as we shall see) this is ultimately an empirical question that depends on the depth of structures at each level of analysis. It is also important to note that the intersubjective basis of social identities can be cooperative or conflictual. What matters is how deeply the social structures they instantiate penetrate conceptions of self, not whether self and other are normatively integrated. The Cold War was a social structure in virtue of which the United States and the USSR had certain identities. These were embodied in "national security worldviews" (in terms of which each defined self and other) and in role positions in a social structure (see Weldes 1993). The content of national interests was in part a function of these structurally constituted identities (as well as of domestic ones). The United States had an interest in resisting Soviet influence in Angola because the Soviets were an enemy and enmity is a *social* relation.

Social identities and interests are always in process during interaction. They may be relatively stable in certain contexts, in which case it can be useful to treat them as given. However, this stability is an ongoing accomplishment of practices that represent self and other in certain ways (Ashley 1988), not a given fact about the world. Rationalism depends on practices having stabilized identities, which we cannot know a priori.

Collective Identity and Action

The ability to overcome collective action problems depends in part on whether actors' social identities generate self-interests or collective interests. Self-interest is sometimes defined so as to subsume altruism, which makes explanations of behavior in such terms tautological. Instead, I shall define self-interest and collective interest as effects of the extent to which and manner in which social identities involve an *identification* with the fate of the other (whether singular or plural). Identification is a continuum from negative to positive—from conceiving the other as anathema to the self to conceiving it as an extension of the self. It also varies by issue and other: I may identify with the United States on military defense but with the planet on the environment. In any given situation, however, it is the nature of identification that determines how the boundaries of the self are drawn.

In the absence of positive identification, interests will be defined without regard to the other—who will instead be viewed as an object to be manipulated for the gratification of the self. (Note that this does not preclude action that benefits others, as long as it is done for instrumental reasons.) This can take more or less virulent forms. The neorealist claim that states define their interests in terms of relative gains assumes that states tend toward the negative end of the identification continuum, whereas the neoliberal claim that absolute gains predominate assumes that states tend toward the center (neither positive nor negative identification). In both cases, however, self-interest stems from a particular representation of the relationship of self to other—from social, not corporate identity. Self-interest presupposes an other.

Though little discussed in recent international relations scholarship, an extensive literature exists on collective identity in sociology, social psychology, philosophy, and even economics (see, e.g., Calhoun 1991; Chase 1992; Jencks 1990; Melucci 1989; Morris and Mueller 1992; Oldenquist 1982; Sen 1985; Taylor and Singleton 1993; Wartenberg 1991).[5] It refers to positive identification with the welfare of another, such that the other is seen as a cognitive extension of the self, rather than independent. Because of corporate needs for differentiation, this identification will rarely be complete (although some people do sacrifice their lives for others), but to the extent that it exists, there will be an empathetic rather than instrumental or situational interdependence between self and other (Keohane 1984, 122–123; see also Russett and Sullivan 1971, 851–52). This is a basis for feelings of solidarity, community, and loyalty and thus for collective definitions of interest. Having such interests does not mean that actors are irrational or no longer calculate costs and benefits but, rather, that they do so on a higher level of social aggregation. This discourages free-riding by increasing diffuse reciprocity and the willingness to bear costs without selective incentives, an effect supported by empirical research in various fields (e.g., Caporael et al. 1989; Dawes, Kragt and Orbell 1990). This is hardly surprising: if collective action depended solely on coercion or selective incentives, it would be a miracle that society existed at all. The state itself is testimony to the role of collective identity in human affairs.[6]

The distinction between alliances and collective security arrangements provides an instructive illustration. Alliances are temporary coalitions of self-interested states who come together for instrumental reasons in response to a specific threat. Once the threat is gone, the coalition loses its rationale and should disband. In contrast, in collective security systems, states make commitments to multilateral action against nonspecific threats. Collective identity is neither essential nor equivalent to such a multilateral institution but provides an important foundation for it by increasing the willingness to act on "generalized principles of conduct" and diffuse reciprocity (Ruggie 1993a). (*Less* identification would be necessary in pluralistic security communities, in which the commitment is merely not to settle disputes by war, and *more*, in amalgamated security communities, in which states join formal unions [Deutsch et al. 1957].) Is NATO merely an alliance or a collective security system? There are good reasons for thinking the latter (Chafetz 1993), but data on collective identification would help us answer this question.

The difficulties of achieving pure collective identity make it unlikely that the motivational force of egoistic identities among states can be eliminated, as the recent debate over the Maastricht Treaty made clear. The tension between particularism and universalism is not specific to international relations, however, being inherent in the relationship of individuals to groups (Brewer 1991; Chase 1992; Wartenberg, 1991). Group membership can be costly to the individual and even lead to his being killed, all the while that it is highly valued. Identification is a continuum along which actors normally fall between the extremes, motivated by both egoistic and solidaristic loyalties. The existence of multiple loyalties is at the heart of the debate over "European identity" and may generate substantial role conflict (Smith 1992). Resolutions of such conflicts are never permanent or fixed, however, and their evolution cannot be studied if we take interests as given. Thus, I am *not* suggesting that collective interests replace egoistic ones as exogenously given constants in a rationalist model but, rather, that identities and interests be treated as dependent variables endogenous to interaction. This would allow us to treat collective action not merely as a problem of changing the price of cooperation for self-interested actors but as a process of creating new definitions of self.

EXPLAINING SELF-INTEREST IN WORLD POLITICS

States, of course, often do define their interests in egoistic terms. Constructivists would emphasize that these are always in process, sustained by practice, but to the extent that practice is stable, the rationalist assumption that interests are given may be useful. To claim this, however, we need to justify that assumption. I shall briefly examine five explanations for state egoism (three domestic and two systemic), arguing that their strength varies historically and often leaves room for collective identity formation.

Domestic Determinants

Perhaps the most fundamental explanation is that self-interest, even if not presocial, stems from the essential nature of states. One might make this argument with reference to human nature, but more relevant here is corporate nature. In a recent critique of constructivism, for example, Jonathon Mercer (1993) draws on the social psychology of intergroup relations to argue that the mere perception of being in a group is sufficient to generate in-group favoritism and out-group discrimination, suggesting that states are cognitively predisposed to be self-interested when they come into contact. This is an important reminder of the importance of corporate identity and interests in constructing social ones. It does not entail permanent group egoism, however, since the boundaries of the self are not inherently limited to corporate identity and scholars in this same tradition have done important work on the conditions under which groups develop common identities (Gaertner et al. 1993).

Second, states typically depend heavily on their societies for political survival, which may induce them to place societal interests before those of other states and treat the latter as instruments for realizing the former. This is undoubtedly pervasive and reflects a general relationship between dependency and identity formation that I shall discuss. Nevertheless, much depends on the nature of state–society relations. Some states depend more on international than domestic society (Jackson and Rosberg 1982; Wendt and Barnett 1993), while others are embedded in societies in which domestic welfare commitments are projected onto altruistic foreign policies (Lumsdaine 1993) and still others are part of democratic societies that identify through shared norms and political culture. To the extent that the boundaries of society are porous, in other words, states might be propelled toward collective identification by "domestic" factors.

These two arguments come together in a third, which focuses on nationalism, that is, a sense of societal collective identity based on cultural, linguistic, or ethnic ties. Nationalism may be in part "primordial" and thus inherent to societies' self-conceptions as distinct groups. In addition, the dependence of states on their societies may be such that they cultivate nationalist sentiments in order to solidify their corporate identities vis-à-vis each other (Anderson 1983). Nevertheless, the depth and exclusivity of national identities varies greatly. German national identity in 1939 was chauvinist and exclusivist, while today it coexists with a significant European component; Serbian identity has moved in the opposite direction, from coexistence with a Yugoslavian identity to chauvinism. This suggests that how nationalism affects state interests should be treated as an open, empirical issue, not assumed a priori to produce egoism inevitably.

None of this is to deny that these (and other) domestic factors often dispose states to be self-interested. The point is that this is socially constructed and historically contingent, not inherent to stateness. Much the same conclusion follows from a consideration of two systemic determinants of state identity.

Systemic Determinants

Neorealists and institutional idealists might each make systemic arguments for treating self-interest as given. Waltz (1979), for example, argues that anarchies are self-help systems in which states that do not think egoistically will be selected out by those that do. This allows him to assume that surviving states will be self-interested, justifying its treatment for subsequent theoretical purposes as exogenously given. In effect, in Waltz's view anarchy so tightly constrains identity formation that the latter becomes uninteresting, which is how his would-be structuralism begets rationalist individualism (Wendt 1987).

The key to this story is the assumption that anarchies are inherently self-help systems, in which actors do not identify positively with each other's security. I have argued elsewhere that there is no necessary connection between the two (Wendt 1992). An anarchy *may* be a self-help system, but it may also be a collective security system, which is not self-help in any interesting sense. Which logic obtains depends on conceptions of self and other; an anarchy of friends is different from one of enemies. If, because of unit-level factors, states threaten each other's security in their first encounter, then competitive dynamics may ensue, generating egoistic conceptions of self. New anarchies may even be particularly susceptible to such outcomes. But if states bring a friendly or respectful attitude to that encounter, then different dynamics of identity formation may ensue. Anarchic structure explains little by itself; what matters is the identities and interests that states bring to their interactions and the subsequent impact of the latter on the former. Self-help presupposes self-interest; it does not explain it. Anarchy is what states make of it.

In contrast to the realist's Darwinian view of world politics as an asocial "system," institutionalists emphasize the norms and shared understandings that constitute international "society" (Bull 1977). The foundation of this society is the principle of sovereignty. Sovereignty is a social identity, and as such, both a property of states and of international society. Its core is a notion of political authority as lying exclusively in the hands of spatially differentiated states, in which sense it is an attribute of the state implying territorial property rights (Ruggie 1983a). Territorial control is only a "right," however, if it is recognized by other states, in which sense sovereignty is an institution. This institution mitigates the dangers of anarchy, helping states to survive that otherwise might not (Jackson and Rosberg 1982; Strang 1991).

Sovereignty has an ambiguous relationship to self-interest. On the one hand, it is a highly open-ended institution compatible with a diversity of interests, by virtue of empowering states juridically to determine their own interests and creating a measure of de facto security in which they have the luxury of doing so. This indeterminateness of sovereignty is important to the argument I shall make about the internationalization of the state. On the other hand, an important rationale for sovereignty is to meet corporate needs for security and recognition. Moreover, it does so in a particular way, namely, by defining rigid spatial boundaries between the rights of self and other. Because of the intimate connection between these rights and corporate needs, states may perceive threats to their ability to exercise such rights as threats to their corporate individuality—in effect, conflating social with corporate identity. One might expect this especially in the early days of a sovereignty regime, when states are still unsure whether their rights will be acknowledged by others. This, along with the fact that by accepting sovereignty, states give up the right to protection by others, will tend to promote egoistic over collective conceptions of interest, or "possessive" over "social" individualism (Shotter 1990).

If and when a society of egoistic sovereigns has been created, it will resist redefinition in more collective terms for at least two additional reasons. First, the need for ontological security motivates actors to hang onto existing self-conceptions because this helps stabilize their social relationships. Second, once Waltz's self-help world is in place, it will reward egoism and punish altruism. Thus, even though a states system is self-help only in virtue of intersubjective knowledge, the latter confronts states as a social fact that resists easy change. Nevertheless, sovereignty may also *promote* collective identity formation in the long run. Like individuals, states do not want to be engulfed by a collective. This concern may be mitigated, however, by being recognized as sovereign, which increases the confidence that others will respect corporate individuality and leaves the decision to join the collective up to each state. In this way, sovereignty may make it "psychologically" possible for states to develop collective attachments (see Wartenberg 1991), paving the way for an internationalization of the state.

To sum up the argument so far, (1) egoistic interests are based on representations of the relationship between self and other and as such are not an essential feature of individuality; (2) these representations are always in process, even if their relative stability in certain contexts makes it possible to treat them as if they were sometimes given; and (3) many factors dispose states toward egoism, but these do not always preclude collective identities. Still, given that international history has produced mostly egoistic states, collective identity formation must start with and overcome that social fact. I turn now to how such an endogenous transformation of Olson's problem might occur.

COLLECTIVE IDENTITY FORMATION AMONG STATES

The forces disposing states toward particularism sometimes confront others disposing them toward collectivism. Collective identities vary by issue, time, and place and by whether they are bilateral, regional, or global. I cannot examine such variations here, nor can I assess the weight of their determinants relative to those of state egoism. Thus, I specifically do *not* impute any directionality or teleology to the historical process. I shall merely identify some causal mechanisms that, to the extent they are present, promote collective state identities, although their impact may be more lumpy than linear. In view of my concern with endogenizing identity change to systemic theory, I shall limit my focus to factors at the systemic level, even though domestic factors may matter, as well. Some of my arguments will repeat claims made by integration theorists (though here applied to a state-centric framework), while others will reflect general principles of identity formation in "structurationist" and symbolic interactionist social theory. I shall differentiate three types of mechanisms by the causal roles they play (structural contexts, systemic processes, and strategic practice) and address two factors under each. The discussion will again be suggestive rather than exhaustive.

Structural Contexts

The structures of regional or global international systems constitute interaction contexts that either inhibit or facilitate the emergence of dynamics of collective identity formation, and as such they play an indirect causal role. Whereas neorealists define structure in material terms, constructivists emphasize intersubjective structure, while leaving room for the emergent effects of material capabilities.

Intersubjective systemic structures consist of the shared understandings, expectations, and social knowledge embedded in international institutions and threat complexes, in terms of which states define (some of) their identities and interests. They vary in thickness or depth, depending on the density and/or salience of interactions for state actors. Many international relations scholars acknowledge a role for such structures only in cooperative settings, implicitly equating intersubjectivity with normative integration and leaving conflict to be treated in materialist terms. In contrast, I would argue that conflicts are also intersubjective phenomena, partly in virtue or *rules* shared by the parties (Bull 1977) but especially in virtue of shared *perceptions* of issues and threat (see Ashley 1987; Walt 1987). The Cold War was fundamentally a discursive, not a material, structure. Whether cooperative or conflictual, intersubjective structures constitute what kind of anarchy states are in: Hobbes's anarchy is constituted by one such structure, Locke's by another, and Kant's by a third. To say that worlds are defined intersubjectively is not to say they are malleable, however, since intersubjective constructions confront actors as obdurate social facts. Sometimes structures cannot be changed in a given historical context. My idealism is that of Durkheim and Mead, not Pollyanna and Peter Pan.

Intersubjective structures help determine how much "slack" exists in a states system for dynamics of collective identity formation to develop. The greater the degree of conflict in a system, the more the states will fear each other and defend egoistic identities by engaging in relative gains thinking and resisting the factors that might undermine it. In a Hobbesian war of all against all, mutual fear is so great that factors promoting anything but negative identification with the other will find little room to emerge. In the Lockean world of mutually recognized sovereignty, however, states should have more confidence that their existence is not threatened, creating room for processes of positive identification to take hold. The ability of states to create new worlds in the future depends on the old ones they created in the past.

Intersubjective structures give meaning to material ones, and it is in terms of meanings that actors act. British nuclear capabilities were a very different social fact for the United States from Soviet nuclear capabilities. Nevertheless, material structures can have sui generis effects on collective identity formation. On the one hand, within a conflictual intersubjective context actors will tend to infer intentions from capabilities, such that the latter may become emergent sources of insecurity (Jervis 1978). The deployment of SS-18s by the USSR was a new threat to the West, even though its meaning was a function of the Cold War. To this extent, material capabilities may be part of the problem in a conflict, inhibiting the emergence of positive identifications. On the other hand, material structure can facilitate the latter when it provides incentives for collective problem solving (as Dan Deudney [1993] argues with respect to nuclear weapons) or is sufficiently asymmetric that powerful states can coerce weaker ones to identify with them (Ikenberry and Kupchan 1990; Wendt and Barnett 1993). Such coercion may produce hegemony in the Gramscian sense, but hegemony is a form of collective identity, one less easily created when material power is equally distributed.

Systemic Processes

Structures are always being reproduced or transformed by practice and thus are not static background conditions for collective identity formation. To the extent that practice does reproduce them, however, they merely inhibit or facilitate collective identity formation. The remaining factors have more direct causal impacts. By *systemic processes* I mean dynamics in the external context of state action. Both of these arguments are long-standing liberal ones. My point is that they relate to identity, not just behavior—a point lost in neoliberalism's rationalist "two-step."

The first process is rising interdependence. This can take at least two forms. One is an increase in the "dynamic density" of interactions due to, for example, trade and capital flows (Buzan 1993; Ruggie 1983a). A second is the emergence of a "common Other," whether personified in an external aggressor or more abstract threat like nuclear war or ecological collapse (see Lasswell 1972, 24). While the one generates "dilemmas of common interests" and the other "dilemmas of common aversions" (Stein 1983), both increase the objective vulnerability and sensitivity of actors to each other (Keohane and Nye 1987) and, with these, the thickness of systemic structures. This reduces the ability to meet corporate needs unilaterally and increases the extent to which actors share a common fate. These changes in the context of interaction will sometimes affect only the price of behavior (as rationalism assumes), but they may also change identities and interests. Indeed, dependency, whether intersubjective or material, is a key determinant of the extent to which an actor's identity is shaped by interaction, which is why a child's development is normally far more influenced by its parents than by others actors. As the ability to meet corporate needs unilaterally declines, so does the incentive to hang onto the egoistic identities that generate such policies, and as the degree of common fate increases, so does the incentive to identify with others. As interdependence rises, in other words, so will the potential for endogenous transformations of identity, with consequences that go beyond those analyzed by rationalists.

A second systemic process that may encourage collective identity formation is the transnational convergence of domestic values. This can take place in various issue areas, but two of the most salient are cultural (the rise of global consumerism) and political (the spread of democratic institutions, welfare statism, concern with human rights, etc.). Societal convergence can result from rising interdependence, in which case, its effects will be hard to separate from the latter. But it may also stem from demonstration effects, diffusion, and "lesson drawing" (in which one society learns from another that one form of social organization is "better" than another). The effect is to reduce the heterogeneity (or increase the similarity) among actors. As with interdependence, this change in the interaction context may sometimes affect only behavior, but it may also change identities and interests. As heterogeneity decreases, so does the rationale for identities that assume that *they* are fundamentally different from *us*, and the potential for positive identification increases on the grounds that "they're no different from us, and if it could happen to them . . ."

Despite the incentives that rising interdependence and societal convergence may create to adopt more collective forms of identity, however, neither is a sufficient condition for such a result. The vulnerabilities that accompany interdependence may generate perceived threats to self-control, and rising similarity may generate fears that the state has no raison d'être if it is not different from others. States may respond to these systemic processes, in other words, by redoubling their efforts to defend egoistic identities. (This may be one reason why students of intergroup relations have found that increased contact alone does not ensure cooperation.) The key to how states deal with the tension between corporate fears of engulfment and the growing incentives for collective identification, therefore, lies in how they treat each other in their changing interaction context.

Strategic Practice

In the last analysis, agents and structures are produced or reproduced by what actors *do*. Systemic structures and processes may affect the context of interaction, but specific actions are rarely dictated by them. Actors sometimes act as though they were in a "game against nature", but more interesting here is strategic practice, in which others are assumed to be purposive agents with whom one is interdependent. This is, of course, the traditional province of game theory, which normally does the rationalist "two-step" of treating identities and interests as exogenous to interaction.[7] Without minimizing the usefulness of game-theoretic models when identities and interests are stable, I want to suggest that more is always "going on" in strategic interaction than such models convey, namely, the production or reproduction of identities and interests. When these change as a result of interaction, game-theoretic models will misrepresent the possibilities for, and mechanisms of, cooperation. In what follows, I shall focus on two forms of interaction differing in their form of communication: behavioral and rhetorical.

The first argument involves a constructivist rereading of Robert Axelrod's (1984) "evolution of cooperation." Taking an iterated, two-person prisoner's dilemma as his model, Axelrod shows how a strategy of reciprocity, or "Tit for Tat," can generate behavioral cooperation. This process becomes attenuated in the n-person case of interest here, where it is harder to target specific others and so may only work in small or "k-groups" within a larger context (Olson 1965). But the general logic is transportable: through repeated acts of reciprocal cooperation, actors form mutual expectations that enable them to continue cooperating. Like Olson, however, Axelrod's basic model assumes that actors remain egoistic,[8] and as such that interaction only affects expectations about others' behavior, not identities and interests. This gives the argument greater generality, applying to pigeons as much as to people, but it brackets the possibility that interaction may transform the interests constituting a game.

In contrast, if we treat identities and interests as always in process during interaction, then we can see how an evolution of cooperation might lead to an evolution of community. This can occur as an unintended consequence of actions carried out merely to realize self-interests or as a result of a conscious strategy of collective self-transformation (Gamson 1992, 60). Repeated acts of cooperation will tend to have two effects on identities and interests. First, the symbolic interactionist concept of "reflected appraisals" suggests that actors form identities by learning, through interaction, to see themselves as others do (Berger and Luckmann 1966; Mead 1934; Rosenberg 1981). The more significant these others are, as measured by the material and/or intersubjective dependency of the self upon them, the faster and deeper this process works. By showing others through cooperative acts that one expects them to be cooperators too, one changes the intersubjective knowledge in terms of which their identities are defined. Second, through interaction actors are also trying to project and sustain presentations of self (Goffman 1959). Thus, by engaging in cooperative behavior, an actor will gradually change its own beliefs about who it is, helping to internalize that new identity for itself. By teaching others and themselves to cooperate, in other words, actors are simultaneously learning to *identify* with each other—to see themselves as a "we" bound by certain norms. As with Axelrod's argument, this process may be attentuated in an n-person context, but the fact that humans do associate in communities suggests that repeated interaction can transform an interdependence of outcomes into one of utility.

Many examples of such transformations might be cited, but a particularly apposite one here is John

Ruggie's (1983b) analysis of the postwar free-trade regime as "embedded liberalism."[9] In keeping with its rationalist basis, conventional regime theory treats the rules, norms, and principles defining regimes as external constraints affecting merely the price of behavior. Ruggie shows that the free-trade regime does more than this, having institutionalized in state–society relations new state identities and interests that conform to the regime, in effect creating new subjects of international relations. Insofar as these define a transnational community of interest, states have incentives for compliance that go beyond the transaction costs of defection. The evolution of such a community can only be explained, however, if we examine the effect of practice on identities and interests.

The discussion so far has focused only on the potential effects of strategic behavior on identities and interests, and as such it is consistent with the concept of a noncooperative game in game theory. This may be realistic in situations where actors do not have available more direct forms of communication (as in the prison of the prisoner's dilemma), but this is not typically the case. What might be called rhetorical practice may have effects similar to those of behavioral practice but it does so through a different mode of communication, variously enacted as consciousness raising, dialogue (Offe and Wiesenthal 1980), discussion and persuasion (Caporaso 1992, 614–6), education, ideological labor (Hall 1986), political argument (Plotke 1992), symbolic action (Johnson 1988), and so on. Despite differences, all of these processes presuppose that the social world is constituted by shared meanings and significations, which are manipulable by rhetorical practices. These practices may involve power, but of the "third-dimensional," rather than the "first-dimensional" kind (Lukes 1974); that is, they are efforts to change others' conceptions of their interests. The goal of rhetorical practices in collective action is to create solidarity; thus they may have an important expressive function independent of their instrumental value in realizing collective goals.

A good part of the "action" in real-world collective action lies in such symbolic work. When leaders of the G-7 hold annual but substantively trivial meetings to discuss economic policy, when European statesmen talk about a "European identity," when Gorbachev tries to end the Cold War with rhetoric of "New Thinking" and a "common European home," when Third World states develop an ideology of "nonalignment," or when the United States demonizes Saddam Hussein as "another Hitler," states are engaging in discursive practices designed to express and/or to change ideas about who "the self" of self-interested collective action is. These practices may ultimately serve an instrumental or strategic function, but they cannot be understood from a strictly rationalist standpoint, since they are at base *about* redefining identity and interest.

Rival Hypotheses

There is nothing inevitable about collective identity formation in the international system. It faces powerful countervailing forces, and I do not mean to suggest that the logic of history is progressive; there are too many examples of failed collective identities for that. My point is only that *to the extent that* mechanisms are at work that promote collective identities, models that ignore them will understate the chances for international cooperation and misrepresent why it occurs. In that sense, my argument is a rival to the rationalist hypothesis about collective action. The precise nature of the rivalry depends on how the latter is formulated—whether as the claim that egoistic state interests are, in fact, given and constant (which instructs us to ignore the formation of interests) or as the claim that states might develop collective interests but only as a result of domestic factors (which leads to reductionism about state interests). Despite the differences between these claims, both treat interaction as affecting only the price of behavior and thus assume that a rationalist research agenda exhausts the scope of systemic theory. My rival hypothesis is not merely that states might acquire collective interests (which dissents from realism) but that they might do so through processes at the systemic level—which dissents from rationalist versions of systemic theory, realist *or* liberal. Indeed, by focusing on the systemic origins of collective identity, I have made systemic rationalism rather than realism my primary target.

Which hypothesis is more appropriate in a given context is an empirical question that may change over time. Two strategies for comparison suggest themselves. One would avoid the direct measurement of identities and interests and focus instead on testing the different behavioral predictions of the two models about how much collective action should occur, and, taking into account problems of revealed preference, on that basis infer whether interests are collective or egoistic. A problem with this strategy is that it is difficult to specify exactly how much collective action even a well-worn theory like Olson's would predict (Green and Shapiro 1994). This strategy also tells us little about the process and causes of collective identity formation. Thus a second strategy would be to focus more directly on identities and interests as the dependent variable and see whether, how, and why they change. The challenge here, of course, is to construct measures of state identity and interest capable of sustaining inferences about change, which I cannot take up here (see Wendt, n.d.).

THE INTERNATIONAL STATE

This essay so far has been a creature of the "anarchy problematique" (Ashley 1988), which theorizes about states in anthropomorphic terms as purposive actors interacting under anarchy. Despite my "sociological" departure from the dominant "economic" vocabulary, like rationalists I treat states as agents having identities, interests, rationality, and so on. There are important objections to this analogy (Ferguson and Mansbach 1991, 370), but I believe it is legitimate for many purposes (Geser 1992; Wendt n.d.). It becomes problematic, however, if it so dominates our thinking that we automatically treat whatever is external to territorial state actors as "not-state" and therefore anarchic. This may obscure the emergence of state powers at the *international* level that are not concentrated in a single actor but distributed across transnational structures of political authority and constitute a structural transformation of the Westphalian states system. Collective identity formation is an essential aspect of such a process.

All but the most hard-core neorealists would probably say that the international system is characterized by "particles" of governance and that as such it is not a pure anarchy (compare Waltz 1979, 114–6). With the deepening and proliferation of international institutions, these particles are rapidly becoming "sediment." There is widespread agreement that this does not represent a move anytime soon toward centralized authority on a "domestic analogy" (Bull 1977), but neither does it represent a persistence of anarchy, since it does involve authority and "governance" (Rosenau and Czempiel 1992). This suggests that the anarchy–hierarchy dichotomy that has organized the field for so long is problematic (Milner 1991) and opens the door to new thinking about the foundations of international politics.

In response to this challenge, some scholars advocate non-state-centric thinking on the assumption that the concept of the state is inherently tied to centralized authority (Ferguson and Mansbach 1991). In contrast, other emphasize the mutability of state forms, differentiating the state from sovereignty and even territoriality (Ruggie 1993b; R. Walker 1990). One way to develop this latter suggestion would be to define the state as, at base, a *structure of political authority* that performs governance functions over a people or space (Benjamin and Duvall 1985; Katzenstein 1990). The enactment and reproduction of this authority structure may or may not be centralized in a single actor. In the Westphalian system, state agents and authority structures did coincide spatially, which leads to the familiar billiard ball imagery of "states" (actors, under which authority structures are subsumed) interacting under anarchy. But the two concepts need not correspond in this way: political authority could in principle be international *and* decentralized (Ruggie 1983b; Pasic 1993). Following Robert Cox, we might call such transnational structures of political authority that lack a single head "international states" (1987, 253–65).[10]

The concept of authority has a dual aspect: legitimacy (or shared purpose) and coercion (or enforcement) (Ruggie 1983b, 198). This suggests that the internationalization of the state requires the development of two qualities: identification with respect to some state function, be it military security, economic growth, or whatever and a collective capacity to sanction actors who disrupt the performance of that function. The result of such developments would be an *institutionalization* of collective action, such that state actors would regard it as normal or routine that certain problems will be handled on an international basis (Nau 1993). This expectation is likely to be expressed and met in various ways: norms, rules, and principles that define expectations for behavior; routine discussions of collective policy; and interorganizational networks among bureaucratic agencies (Geser 1992; Hopkins 1978). By themselves, these forms are not international states, but to the extent that they manifest and contribute to collective identity and enforcement, they will be part of an internationalization of political authority.

This is present today in various degrees among advanced capitalist countries in both security and economic issues. The provision of security (and more specifically the maintenance of a territorial monopoly on organized violence) is a key function of the state. "Monopoly" normally refers to control by a single actor, but it can also denote control by multiple actors if they are not rivals and engage in institutionalized collective action. A collective security system is just that—*joint* control of organized violence potential in a transnational space. Such control with respect to external security has long characterized NATO, and recently European Community states have begun to internationalize internal security as well (Den Boer and Walker 1993). This system has a high degree of legitimacy among its members, as well as some capacity for enforcing its policies on them. It is based on multilateral norms that give even its weakest members a say in decisions (Risse-Kappen 1991), its leaders routinely formulate policies together, and its militaries are linked organizationally for both operations and procurement. Its capacity for institutionalized collective action is certainly also lacking in many respects, as the recent difficulties of defining a European defense policy attest, but the internationalization of political authority is a continuum, and NATO is far from anarchy along it.

The provision of an institutional framework for capitalist production is another function of the state that is today being internationalized. Historically, capitalism was largely a territorial phenomenon, in which capitalists could direct their political efforts toward corresponding domestic authorities. As competition drove them to expand overseas through trade and investment, however, they created a demand for international rules and regulations. State actors responded with a network of regimes: a trade regime to govern the flow of goods and services; a monetary regime to govern the value of transactions; and, increasingly, a "capital regime" to govern property rights and capital flows (Duvall and Wendt 1987). Today these regimes do more than merely affect the prices of certain behaviors; they embody a degree of collective identity ("embedded liberalism"), routinized discussions of collective policy, and networks of interorganizational linkages. Their principal weakness remains enforcement, but even here there are emerging sanctioning systems that enable us to speak of an internationalization of political authority with respect to global capital (Cox 1987).

Let me emphasize that I am *not* saying that international state formation has gone very far, any more than has the formation of collective identities that is one of its prerequisites. It is a process, and even if it continues we are only in its early stages. It is issue-specific (though it may "spill over" into new issue areas), mostly regional in nature, and a matter of degree. Moreover, there are strong arguments for thinking that it will not continue, since it creates fundamental tensions between the national and transnational responsibilities of state actors (Taylor 1991). My point is merely to suggest some ways of thinking about certain dynamics in the contemporary world system that do not privilege the dichotomy between anarchy and hierarchy in statist theory (or its counterpart of intergovernmentalism versus supranationalism in integration theory). The key to such thinking is recognizing that political authority need not be centralized, a point recognized implicitly long ago by the integrationists and, since the neorealist interregnum, it is being rediscovered by a variety of scholars today (Pasic 1993). To be sure, centralization or supranationality *may* facilitate an internationalization of political authority, and the latter may even create some incentives for it. But one might also argue that *de*centralized governance arrangements will facilitate the process, since by preserving the forms if not substance of sovereignty, they may pose less tangible political threats to state actors against which resistance might coalesce.

The internationalization of political authority has at least two broad implications for international relations theory. First, it points toward a gradual but structural transformation of the Westphalian states system from anarchy to authority. The basis of that system is the institution of sovereignty, which constitutes an anarchy of mutual recognition. Even when international state formation does not involve the formal cession of sovereignty to supranational institutions, it does relocate individual state actors' de facto sovereignty to transnational authorities. The result in practice might be a "disarticulated" sovereignty in which different state functions are performed at different levels of aggregation (Pogge 1992), and/or a "neo-Medievalism" in which political authority is shared by both state and nonstate actors (Bull 1977, 254–94). Either way, the result is neither anarchy nor hierarchy but the emergence of a new form of state—and thus states system—which breaks down the spatial coincidence between state-as-actor and state-as-structure. Thus the erosion of individual state sovereignty does not imply the erosion of the state. Sovereignty is not an intrinsic feature of state agency but one social identity a state may have. By transferring it upward to a collective, states may actually strengthen their capacity to solve problems. Internationalization is a way of reorganizing and redeploying state power, not a withering away of the state.

A second implication is how this calls into question the premises of contemporary democratic governance. The Westphalian approach to sovereignty allowed democratic and international relations theorists to ignore each other. The former were concerned with making state power democratically accountable, which Westphalia constituted as strictly territorial and thus outside the domain of international relations theory; the latter were concerned with interstate relations, which were anarchic and thus outside the domain of political theory. Under this worldview, democrats could celebrate the "end of history" with hardly a peep about democracy at the *international* level.

The internationalization of the state makes this silence problematic.[11] As state actors pool their de facto authority over transnational space, they remove it from direct democratic control. Territorial electorates may still retain the formal right to "unelect" their leaders, but the ability to translate this right into tangible policy change (versus merely changing the faces in power) is constrained by the commitments that state elites have made to each other. New elites could in principle break those commitments, but often only at the cost of external sanctions and reductions in their own effectiveness (Dahl 1993). Various interpretations of the threat this poses to territorial democracy can be imagined. The current debate in the European Community over the "democratic deficit" (Williams 1991) treats it in largely liberal terms as one of controlling an emerging centralized power, but radical democratic theory might be more relevant for the more decentralized authority structures cropping up all over the international system. In either case, the attempt to solve international collective action problems by creating collective identities among states creates an entirely new problem of making those identities democratically accountable, a problem ultimately of transforming the boundaries of political community (Linklater 1990; Wolfe 1992). Solutions might take transterritorial or functional–corporatist forms but will somehow have to expand "the people." In contrast to their splendid isolation in the sovereignty regime, democratic and international relations theorists might want to work on this one together.

CONCLUSION

It is widely thought that state-centric systemic international relations theory cannot explain structural change and so ought to be abandoned. In my view, the problem lies not with statism but with two other commitments that inform contemporary understandings of structural theory: realism and rationalism. The essence of the former is materialism, *not* a willingness to confront the ugliness of world politics as its proponents would have it. If system structure is reduced to a distribution of material power, structural change can mean nothing more than shifts in polarity that will not end the dreary history of conflict and despair over the millennia (but see Deudney 1993). Since authority is an intersubjective concept, the nature and implications of its internationalization will elude a materialist theory. The essence of rationalism, in contrast, is that the identities and interests that constitute games are exogenous and constant. Rationalism has many uses and virtues, but its conceptual tool kit is not designed to explain identities and interests, the reproduction and/or transformation of which is a key determinant of structural change. We should not let our admiration for rationalist methodology dictate the substantive scope of systemic international relations theory (Shapiro and Wendt 1992).

Scholars should not ignore the state as they grapple with explaining structural change in the late twentieth century, but for their part, statists should recognize that when states interact, much more is going on than realism and rationalism admit. Yes, international politics is in part about acting on material incentives in given anarchic worlds. However, it is also about the reproduction and transformation—by intersubjective dynamics at both the domestic and systemic levels—of the identities and interests through which those incentives and worlds are created. Integration theorists appreciated this suggestion long ago, but their nascent *sociology* of international *community* has been lost in the *economics* of international *cooperation* developed by realists and rationalists. The latter has contributed important insights into the dynamics of interaction under anarchy, but is ill-suited as a comprehensive basis for systemic theory precisely because it brackets some of the most important questions such a theory should address. Constructivists bring renewed interest and sharpened analytic tools to those questions.

Notes

An earlier version of this article was presented at the 1993 meeting of the APSA in Washington. The article would not have been possible without the many helpful comments of Emanuel Adler, Mike Barnett, Lea Brilmayer, Mlada Bukovansky, James Caporaso, Martha Finnemore, Mark Laffey, Roy Licklider, David Lumsdaine, James Marino, Jonathon Mercer, Debra Morris, Bruce Russett, Rogers Smith, and seminar participants at Cornell University, Johns Hopkins University, New York University, University of Pennsylvania, Rutgers University, and Yale University; thank you.

1. For important exceptions, see Nye 1987 and Jervis 1988.

2. For varying expressions of this argument, upon which I draw freely, see Adler 1991; Alker n.d.; Ashley 1987; Katzenstein 1990; Kratochwil 1989; Ruggie 1993b; R. Walker 1990; and Wendt 1992.

3. Other traditions of international relations theory that might fall under this heading include poststructuralism, world society theory, neo-Gramscianism, Deutschian integration theory, and perhaps the society of states school. For elaboration of this and other issues discussed in this section, see Wendt n.d.

4. For a useful discussion of the importance and nature of basic needs at the individual level see Turner 1988, 23–69.

5. As noted, collective identity was a centerpiece of classical integration theory, but the latter's insights have been pushed aside by the realist and rationalist approaches that dominate contemporary international relations theory. For recent international relations scholarship that begins to revive the concept, see Alker n.d. 1986; Caporaso 1992, 617–20; Keohane 1984, 120–34.

6. Although collective identification operates at the level of social identity, by enhancing the capacity for collective action it can help create corporate identity, which in one sense is simply a (temporarily) "solved" collective action problem. The distinction between the corporate and social identity of groups is therefore itself a construction, signifying the higher self-organization of the former rather than timeless and essential.

7. An interesting exception is the rationalist literature on "endogenous preference formation"; see Cohen and Axelrod 1984.

8. Later in his book, Axelrod discusses the possibility of actors internalizing new interests, but this important point has not been picked up by mainstream regime theorists in international relations.

9. Another is Schroeder's (1993) splendid discussion of how the European great powers learned to think in "systemic" (in my terms, essentially "collective") terms in the early 1800s.

10. This concept has been used primarily by neo-Marxists (Cox 1987; see also Picciotto 1991) though see the interesting discussion of Kant's usage in Hurrell (1990). My own thinking on this score, as on many others, owes much to conversations and work with Bud Duvall (see esp. Duvall and Wendt 1987).

11. For an early appreciation, see Kaiser 1971; the recent literature includes Connolly 1991; Held 1990; and R. Walker 1991.

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RESEARCH NOTES

REASSESSING MASS SUPPORT FOR POLITICAL AND ECONOMIC CHANGE IN THE FORMER USSR

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Using survey data collected in Russia, Ukraine, and Lithuanian in 1990–92, we reexamine findings reported by Finifter and Mickiewicz (1992). Our analysis indicates a significant link between political and economic reform orientations. Individuals who prefer political reforms of a democratic nature also favor a decreased role of the state in guaranteeing social well-being. In addition, our finding that better-educated Soviet and post-Soviet citizens are more likely than the less-well-educated to prefer individual responsibility for well-being is in direct contradiction to the findings reported by Finifter and Mickiewicz. The differences in the two sets of conclusions give rise to very different substantive conclusions regarding the economic and political changes now occurring in these societies.

Finifter and Mickiewicz (1992) present data from a survey of public opinion in the former USSR. They use these data to analyze some of the critical issues in the transition toward new political arrangements, including popular views of whether the state or the individual should be responsible for securing the well-being of the citizenry, as well as popular orientations toward political and economic reform. Their article warrants attention from a wide audience because it presents data from the "first large-scale true personal interview survey in the Soviet Union" (p. 859), because of the centrality of the theoretical issues they address, and because of some unexpected findings. They conclude, for example, that those who have obtained a higher level of education, as compared with the less-well-educated, may be more likely to prefer a strong governmental role in promoting social well-being. They also find significant similarity in the political and economic attitudes expressed by different ethnic subgroups across various republics of the former Soviet Union. Both of these findings are surprising in light of conclusions drawn earlier from the Soviet Interview Project and from research based outside of this region.

Given the importance of the issues that Finifter and Mickiewicz tackle, it is crucial to compare their findings with those obtained from other data so as to prevent preliminary conclusions from becoming accepted as "received wisdom" before they have been verified. Therefore, we reexamine several key arguments put forth by Finifter and Mickiewicz using data both from a series of three large-scale surveys conducted by the University of Iowa and the USSR Academy of Sciences (now Russian Academy of Sciences) in 1990–92 and from American National Election Studies (NES) data, which permit a broader cross-systemic comparison. Based on these analyses, we are able to reinforce and confirm some of their findings, but more importantly, our results contradict several notable conclusions reached by Finifter and Mickiewicz.

Specifically, our reexamination of these issues demonstrates that the proportion of those who believe that the individual, as opposed to the government, should be responsible for producing social well-being is rather close to the levels found among Americans. The average level of this individualism increased from 1991 to 1992, thereby casting doubt on the argument that 70 years of societywide Soviet indoctrination (although the socialization was admittedly less than fully effective) account for the levels found. In addition, differences in support for individual versus governmental responsibility are influenced significantly by variables (such as income and gender) that Finifter and Mickiewicz found to be unrelated to locus of responsibility and, even more significantly, by variables that they did not even include in their analyses (personal economic situation and rating of the Communist party). Furthermore, the better educated are *more* likely than the less-well-educated to prefer individual responsibility for well-being over governmental responsibility, thus directly contradicting the findings reported by Finifter and Mickiewicz. Another contradiction involves popular support for political reform, which, contrary to Finifter and Mickiewicz, is influenced by the respondent's nationality. Finally, our data call into question Finifter and Mickiewicz' argument that attitudes toward the locus of responsibility and toward political reform represent distinct dimensions, thereby permitting economic and political reform to move in different directions. Rather, our analyses show strong, positive correlations between the two even when controlling for other key factors.

The discussion and data analysis supporting these contrary conclusions proceed by first examining more fully the concept and measurement of locus of responsibility. Then we turn to testing the four major hypotheses guiding the analysis. The first hypothesis involves reexamining the relationship between education and locus of responsibility. Our expectation is that the better-educated will be more supportive of individual responsibility. Second, using a multivariate analysis we investigate the hypothesis that political and economic conditions influence preferences for individual rather than governmental responsibility for well-being. Third, to test the hypothesis that nationality differences are a factor influencing political and economic attitudes, we include nationality as one of the independent variables predicting the locus of responsibility and political reform measures. The expectation here is that attitudes toward political reform should be influenced more strongly by nationality differences than are economic orientations toward the locus of responsibility. Finally, we examine the fourth hypothesis, which involves an expected relationship between attitudes toward political and economic reform.

Our analysis differs from that of Finifter and Mickiewicz in certain respects. Because we have data from only three former republics (Russia, Ukraine, and Lithuania), we cannot fully reproduce their analyses. We can, however, offer a more in-depth understanding and analysis of the relevant hypotheses in these three countries because our sample was designed to be representative of each of these former republics. (See the Appendix for a description of the data.) The Finifter and Mickiewicz sample, on the contrary, was not designed to be representative of particular republics but was supposed to represent the broader Soviet Union. Thus very few respondents were found in any given republic, except Russia. Also, with regard to questionnaire construction, although several of the questions asked by Finifter and Mickiewicz were very similar to those asked in the University of Iowa surveys, none of the items that they reported employed exactly the same wording as was used in our interviews. Because specific survey questions are slightly different, we do not reproduce any of their earlier published tables here for direct comparison. We do encourage the reader, however, to make those direct comparisons on their own. Also, to avoid problems associated with attempts to make direct comparisons across individual questionnaire items, we will employ multiple-item scales to tap equivalent underlying concepts. Our time frame is also different: the Finifter and Mickiewicz interviews were undertaken only once, at the end of 1989, while our interviews were administered in three annual waves beginning in the summer of 1990 and finishing in the summer of 1992. Because the 1990 survey included fewer survey items relevant to the analysis presented here, we will draw most heavily from the 1991 and 1992 survey data.[1]

LOCUS OF RESPONSIBILITY FOR WELL-BEING

As Finifter and Mickiewicz note, a core element of socialist ideology involves the locus of responsibility for social well-being. In order to gauge public support for ideological tenets, we focus on public perceptions of who should be responsible for individual economic well-being: the state or the individuals themselves? Under socialist systems, the state has assumed the major role in caring for individual well-being. As a matter of ideology (and also practice), the socialist state controls property, the means of production, and most aspects of the economy that directly influence one's economic well-being. Socialization within this system, therefore, could be expected to promote both an expectation that the state will continue to provide for social well-being and a preference for the state to assure a high standard of living for every citizen. On the other hand, preferences for individual responsibility are likely to be more prevalent in systems characterized by relatively more laissez-faire market economies. Given these cross-system expectations, the distribution of public attitudes toward the locus of responsibility for the former Soviet Union is of considerable theoretical importance.

The distribution of responses to the question on locus of responsibility for a sample of Russians, Ukrainians, and Lithuanians interviewed shortly before the August 1991 coup attempt and one year later are presented in Table 1. Comparing the responses of the total sample from the 1991 and 1992 studies to those reported by Finifter and Mickiewicz reveals a great deal of similarity. The comparison cannot be made directly, however, because the question asked in our surveys had a middle (pro/con) category, whereas their individual-versus-state-responsibility indicator was dichotomous. But when the percentages for our data are recalculated after excluding the middle category, the distributions are nearly the same as what Finifter and Mickiewicz report. In 1991 only slightly more than half (53.7%) preferred state responsibility and in 1992 the division was virtually the same as the 50.7% found by Finifter and Mickiewicz (see Table 1). Across the three former republics, only Russians displayed a statistically significant, although slight, shift toward individualism between 1991 and 1992. Lithuanians, however, expressed more individualism than did Russians or Ukrainians in both years, thereby confirming a finding reported by Finifter and Mickiewicz. Despite the collapse of the Soviet Union at the end of 1991, these attitudes, at least at the aggregate level, appeared quite stable across time. At first glance, therefore, this stability may be attributed to basic, enduring socialization.

Comparable data from the NES, however, provide a cross-national comparison suggesting that the distribution of preferences may have less to do with ideological indoctrination than with current economic circumstances and realistic judgements regarding what the Soviet state and successor governments could be expected to do under existing economic conditions. The 1988 NES dichotomous distribution excluding the middle category (see Table 1) had 64.1% preferring individual responsibility, signifi-

TABLE 1

State or Individual Should Be Responsible for Job and Standard of Living

RESPONSIBILITY FOR ECONOMIC WELL-BEING	TOTAL		RUSSIA		UKRAINE		LITHUANIA		UNITED STATES	
	1991	1992	1991	1992	1991	1992	1991	1992	1988	1990
Trichotomized[a]										
State (%)	40.7	36.6	41.4	35.5	39.8	38.1	35.0	35.0	28.3	35.5
Pro/con	24.2	27.8	25.7	30.2	21.6	25.6	21.8	28.5	21.3	21.1
Individual	35.1	35.6	32.9	34.3	38.6	36.3	43.2	36.5	50.4	43.3
N	2,911	2,580	1,384	1,243	953	845	574	492	1,725	1,710
Dichotomized[b]										
State (%)	53.7	50.7	55.7	50.9	50.8	51.2	44.7	48.9	35.9	45.0
Individual	46.3	49.3	44.3	49.1	49.2	48.8	55.3	51.1	64.1	55.0

Source: University of Iowa's Post-Soviet Citizen Surveys.

[a] The survey question scale initially ranged from 1 = state responsibility to 5 = individual responsibility for economic well-being. Here we have collapsed the scale as follows: 1–2 = state, 3 = pro/con, 4–5 = individual. The U.S. data employed a seven-point scale, so the collapsing was 1–3 = state, 4 = pro/con, 5–7 = individual.

[b] In this collapsing of the initial scale, we eliminated those responding 3 = pro/con and recalculated the percentages, still having 1–2 = state, 4–5 = individual. The U.S. data also eliminated the center category.

cantly different from the 46.3% found in the Soviet Union for 1991. Moreover, the difference fits with the theoretical expectations for the two countries (i.e., Socialist countries showing the stronger orientation toward state responsibility). The situation changes, however, in 1990, when the United States was in a deep recession and many more people were expressing the feeling that the state, rather than individuals, should provide for social well-being (see Table 1). Clearly the difference between U.S. and Soviet attitudes on locus of responsibility is much smaller when the 1990 U.S. data are used in the comparison.

What is surprising about these data is not the fact that Soviets were so evenly divided between state and individual responsibility for individual well-being but that a relatively high percentage of Soviets (as compared to Americans) preferred an individual solution. After 70 years of assumed socialist indoctrination and a tendency for people to turn to the state for help in periods of economic crisis, we would have expected that a much higher percentage of Soviets would view the state as the preferred provider of social well-being. Of course, given that the distributions in Table 1 rely upon a single survey item to measure locus of responsibility, the critic might be rightfully skeptical of these results. Fortunately, the surveys we are employing as evidence each include several questions designed to measure the concept of locus of responsibility. Therefore, we can expand the analysis and are not limited to a single item (as were Finifter and Mickiewicz). Indeed, an exploratory factor analysis revealed that four items from our 1991 and 1992 surveys formed a cohesive, moderately intercorrelated set of questions that could be used in an index that ranged from strong preferences for state responsibility to strong preferences for individualism.[2] The set of four items included the variable discussed in Table 1, a question on concerns about economic inequality, an item measuring preferences for maintaining state or collective property versus the establishment of private property, and a question on attitudes toward "businessmen" (understood in the Soviet context to mean entrepreneurs). The text of the questions are presented in the Appendix.

Briefly, the pattern of responses to the three questions not already discussed tends to indicate a slight shift in an individualistic direction between 1991 and 1992. For example, 43% of the total respondents in 1991—as compared with 35% in 1992—felt that economic inequality was a grave danger to social stability. Also, 43% of the 1991 respondents said they had nothing in common with businessmen, whereas in 1992 that figure declined to 40%. On the question of owning property, among the 1991 sample 33% preferred state or collectively owned property, while 31% favored private property (the remainder wanted some combination). In 1992, the data revealed only 26% favoring state-owned property, while 43% preferred private property. This overall shift in the direction of individualism is also clearly evident in the mean value of the four-item locus of responsibility index. (Overall, the mean shifted from 6.72 to 7.13, where high values indicate support for individualism.) The index means also differed across republics. In both years, Lithuanians were more individualistically oriented (7.77 and 7.84 in 1991 and 1992 respectively) than Russians or Ukrainians (means of 6.67 and 6.73 for the two countries in 1991 and 7.07 and 6.83 in 1992), a difference that is not surprising given Lithuania's historically closer ties with Europe and their early movement to secede from the Soviet Union. In 1991, Russian and Ukrainian preferences were not significantly different. However, the most rapid across-time shift in these attitudes occurred among Russians. By 1992, therefore, Russians were more individualistically oriented than Ukrainians.

TABLE 2

Percentage Saying the Individual Should Be Responsible for Well-Being by Level of Education (%)

EDUCATION LEVEL	FORMER SOVIET UNION		UNITED STATES	
	1991	1992	1988	1990
Secondary education	19.2	24.7	41.1	33.7
Higher education	43.4	45.7	56.8	49.4
Pearson r	.19*	.15*	.13*	.12*

Source: University of Iowa's Post-Soviet Citizen Surveys.
Note: Table entries are the percentage giving the individual response to the trichotomous version of the question presented in Table 1. Education is dichotomized here into those with secondary education (or less) versus those with more-than-secondary education.
*$p < .001$.

EDUCATION HYPOTHESIS

Our major concern with the Finifter and Mickiewicz report, however, is not the distribution of responses to the locus-of-responsibility questions. On the contrary, in the case where we have comparable questions, the estimate of the distribution derived from their data and that presented in Table 1 are very similar. Rather, the major problem is how these attitudes on responsibility for well-being are related to other variables, particularly such demographic variables as education and income.

Most surprising is the negative correlation they find with education, namely, that the better-educated favored state responsibility for well-being, whereas the lesser-educated favored individual responsibility (Finifter and Mickiewicz, 1992, 866). As Finifter and Mickiewicz point out, this negative correlation is contrary to that found earlier with data on Soviet emigres (Silver 1987). In addition, Duch recently found a correlation between education and endorsement of free-market culture variables (1993, 102) that was the opposite of the finding reported by Finifter and Mickiewicz (although we are not arguing here that the measures of free-market culture and locus of responsibility are tapping the same attitudes). The negative relationship reported by Finifter and Mickiewicz also runs counter to the correlation obtained with the single-item measure in our 1991 and 1992 data and even contradicts the relationship found with U.S. data (see Table 2). Moreover, the correlation between the four-item locus-of-responsibility index and education is positive and significant (.19 and .15 in 1991 and 1992 respectively). Given this overwhelming evidence, one can only conclude that the better-educated in both the former Soviet republics and in the United States are significantly more likely to endorse individual responsibility for providing a high quality of well-being than are the lesser educated. Thus, the results in Table 2 are not only consistent across time and cross-culturally, but they better match theoretical expectations than those presented by Finifter and Mickiewicz.

The interpretation that Finifter and Mickiewicz brought to the education relationship they found was twofold. First, they argued that the better-educated were really the ones who were benefiting from the material and psychic gratification provided by the Soviet state (1992, 866). Second, the better-educated were altruistically concerned about providing a safety net for the less fortunate in society, so that they were more supportive of the state bearing the burden of providing social welfare for its citizens (p. 867).

The data in Table 2, however, suggest a very different interpretation. It is more likely that the less-fortunate in the former Soviet societies realized their dire need for state support simply to survive. The better-educated, on the other hand, preferred a system that allowed for more individual initiative and entrepreneurship either because of materialistic motivations or because they desired more freedom and self-determination. Since they were more likely to benefit from a system that gave more emphasis to individualism, they were also more willing to accept greater individual responsibility for their own well-being.

ECONOMIC AND POLITICAL CIRCUMSTANCE HYPOTHESES

The analysis presented by Finifter and Mickiewicz, however, went well beyond the correlation of education and locus of responsibility. To explain the variation and differences in locus of responsibility, Finifter and Mickiewicz utilized a multivariate regression analysis (1992, 869, Tbl. 8) incorporating a number of demographic variables and a measure of life satisfaction. A comparable analysis using our 1991 and 1992 data is presented in Table 3 (cols. 1–2, 5–6). As might be expected with a dependent variable that is a multiple-item index and thus is more reliable and has less-restricted variance than a single question, the explained variance for the equations presented in Table 3 is three times the magnitude of that found by Finifter and Mickiewicz. More importantly, only two of the six (excluding ethnicity controls) independent variables included in the equations—age and life satisfaction—exhibit relationships with the locus-of-responsibility measure that confirm the Finifter and Mickiewicz analysis. In both 1991 and 1992, younger Soviets and those with a more positive sense of life satisfaction were more likely than older and dissatisfied people to prefer individual orientations to government responsibility for social well-being. The regression coefficients for these two variables are similar to those displayed by Finifter and Mickiewicz (1992, tbl. 8).

The major discrepancies between the Finifter and Mickiewicz study and the data reported here arise with respect to education, gender, income, and ethnicity. The contradiction involving the education relationship was mentioned earlier. It is sufficient,

TABLE 3

Regression Equation Predicting Locus of Responsibility for Social Well-Being

INDEPENDENT VARIABLES	1991				1992			
	b	BETA	b	BETA	b	BETA	b	BETA
Personal Finances[a]								
Past	—	—	.41** (.03)	.10	—	—	.74** (.03)	.18
Future	—	—	.56** (.03)	.14	—	—	.53** (.03)	.14
National economy past[a]	—	—	.01 (.05)	.00	—	—	.18 (.05)	.02
Rating of CPSU[b]	—	—	−.69** (.02)	−.28	—	—	−.53** (.02)	−.22
Life satisfaction[c]	.30** (.02)	.09	.23** (.02)	.07	.61** (.02)	.18	.17* (.02)	.06
Age	−.05** (.00)	−.28	−.03** (.00)	−.19	−.05** (.00)	−.29	−.04** (.00)	−.23
Education	.16** (.01)	.12	.16** (.01)	.11	.09* (.01)	.06	.07* (.01)	.06
Gender[d]	.82** (.04)	.14	.71** (.03)	.12	.71** (.04)	.12	.56** (.03)	.10
Urban/rural	−.33* (.04)	−.05	−.16 (.04)	−.03	−.06 (.02)	−.04	−.07 (.02)	−.02
Income	.00* (.00)	.05	.00* (.00)	.05	.00* (.00)	.15	.00* (.00)	.12
Ethnicity								
Ukrainian	.07 (.04)	.01	.13 (.04)	.02	−.34 (.05)	−.05	−.45 (.04)	−.05
Lithuanian	1.47** (.14)	.07	.81 (.13)	.04	.54 (.14)	.03	.19 (.14)	.01
Constant	13.6		16.09		14.8		17.7	
Adjusted R^2	.15		.24		.19		.27	
Number of cases	2,437		2,411		2,197		2,153	

Source: University of Iowa's Post-Soviet Citizen Surveys.

Note: The locus-of-responsibility index ranges from 1 to 15, where 1 indicates state responsibility and 15 indicates individual responsibility for well-being. Standard errors are in parentheses. b = unstandardized regression coefficient, beta = standardized coefficient.

[a] Better off = high.

[b] Positive = high.

[c] Satisfied = high.

[d] Male = high.

*$p < .01$.

**$p < .001$.

therefore, to note that the additional controls for other variables do not change the relationship observed in Table 2; that is, better-educated Soviets favored individual solutions to attaining well-being, while the lesser-educated were more likely to look to the state—exactly opposite to the finding reported by Finifter and Mickiewicz. Furthermore, Finifter and Mickiewicz reported that gender and income had no significant impact on locus of responsibility. The data in Table 4, on the contrary, reveal a consistent and relatively strong effect for gender: in both years, women were more likely than men to favor the state as the provider of social well-being. Likewise, Soviets with lower incomes, particularly in 1992, expressed the stronger preferences for the state as the guarantor of social well-being. Regarding nationality differences, Lithuanians were more supportive of individual responsibility than were Russians and Ukrainians. However, the impact of nationality will become more meaningful when we return to a broader discussion of this variable.

Except for life satisfaction, all the predictors included in the Finifter and Mickiewicz analysis were social-structural variables. As they point out, life satisfaction was included because it has been seen as a cultural attitude that influences the development of political democratization and economic reform (Inglehart 1990). We shall not here attempt to discuss or empirically examine a number of other theoretically relevant political and economic variables that go well beyond the largely demographic variables utilized by Finifter and Mickiewicz. We point out, however, that none of the variables used by Finifter and Mickiewicz indicate the extent to which locus of responsibility was a response to recent change in economic conditions. Also, none of their variables are explicitly political in nature, so they failed to capture the potential impact of contemporary political circumstances. In an effort to address these limitations, we shall briefly extend our investigation to an analysis of how public response to the economic and political circumstances of the time may have affected locus of responsibility orientations.

Earlier findings from political-economy research suggest that both economic and political circumstances could potentially influence public attitudes toward the role of government in guaranteeing the basic economic well-being of citizens, as well as support for a particular type of economic system. (For extensive reviews of this literature, see Kiewiet 1983 and Lewis-Beck 1988.) This research implies that perceived changes in national economic circumstances (sociotropic considerations) will influence collective economic evaluations more strongly than personal or "pocketbook" concerns (Kinder and Kiewiet 1979). Additionally, the earlier research suggests that prospective expectations of how economic conditions may develop in the future can influence political and economic orientations as much as retrospective assessments (Fiorina 1981; MacKuen, Erikson, and Stimson 1992; Miller and Wattenberg 1985).

Duch has recently applied these various arguments to an analysis of survey data collected in the former Soviet Union in 1990 (1993, 591). In doing so, he hypothesized that Soviet citizens who were experiencing a deterioration in their own economic situation or perceived the national economy as deteriorating, as well as those who expected these economic failures to continue in the future, would be more likely to support free-market changes because they would blame the command economy for the current and future economic woes. This same argument could be extended to incorporate the impact of economic circumstances on attitudes toward the locus of responsibility. Also, if the Soviet regime was being blamed for economic failures, both national and personal, then we should expect that those citizens who were most negative toward communists should be most supportive of economic reform. We would hypothesize, therefore, that anticommunist sentiment should promote support for an individually oriented locus of responsibility throughout the late 1980s and early 1990s.

However, by 1991—and certainly by 1992, after the collapse of the Soviet Union—the impact of deteriorating economic conditions may have taken a different turn than that hypothesized by Duch (1993). Under *perestroika* (Gorbachev's economic restructuring program), a good deal of lip service was given to new economic freedoms and the encouragement of new entrepreneurship. Indeed, by 1991, there were emerging signs of these changes. For example, citizen-owned cooperatives were starting to spring up. The collapse of the Soviet Union at the end of 1991 also produced a dramatic increase in the possibility for new entrepreneurship. Real economic conditions continued to deteriorate, but the possibility for individual economic improvement changed significantly during this period. An alternative hypothesis, therefore, is that those who were hardest hit by the collapsing economy needed to look to the state for assistance, whereas those who were actually doing equally well or better during this period, relative to the past, could only hope that they would do even better under a market-oriented, private enterprise system that gave more emphasis to individual responsibility.

The empirical evidence from 1990 to 1992 confirms that this was a period in which former Soviet citizens perceived a dramatic deterioration in their economic circumstances. The percentage of respondents reporting that their personal financial situation worsened "relative to the past year" rose from 40% in 1990 to 56% in 1991 and 61% in 1992. Over this same period, only 8–10% said their economic situation had improved relative to the past. Nonetheless, expectations regarding future economic circumstances actually became slightly more optimistic during this period. In 1990, when respondents were asked to look ahead one year, 63% said they would be worse off in the future. By 1991, respondents giving that pessimistic assessment had dropped to 49% and it fell to 45% in 1992. At the same time, respondents saying they would be better off in the future rose from 7% in 1990 to 11% and 15% in 1991 and 1992 respectively. Likewise, over this period of time former Soviet citizens were increasingly negative in their evaluations of the Communist party. Ratings of the communists became sharply more negative between 1990 and 1991 (rising from 49% to 60% rating communists negatively in the two respective years) and remained predominantly negative through 1992, when 62% rated the communists negatively.

Clearly economic and political circumstances in the former Soviet Union changed significantly between 1990 and 1992—but with what impact on broad economic orientations such as the preferred locus of responsibility? To answer this question and to test the hypotheses we have raised, the regression equations presented in Table 3 were recomputed after adding four new predictor variables to the analysis. The first two items are the retrospective and prospective assessments of one's personal financial situation. The third measure indicates the respondent's judgment of whether the national economy had improved or worsened during the past year, while the fourth measures evaluations of the Communist party on a five-point scale.

The resulting coefficients demonstrate that both personal economic change measures and attitude toward communists had a strong impact on preferred locus of responsibility in 1991 and 1992. Statistically, these new variables contribute nearly 10% more explained variance beyond that provided by the original set of variables in Table 3. The results suggest that changing economic conditions had a major impact on attitudes toward responsibility for economic well-being. Between the times when the two surveys were in the field, Russian president Boris Yeltsin and his administration undertook a series of governmental measures that liberalized prices, ended government subsidies of some industries, and capped some wages. The result was hyperinflation that reduced or eliminated many people's savings and replaced limited supplies of cheap goods with readily available goods that few could afford. To maintain an adequate standard of living for one's family required most adults to scramble for extra income. This new challenge produced tension, fears for the future, and (for some) a higher appreciation of Soviet-era conditions. Those who succeeded in securing adequate income to keep pace with inflation were likely to enjoy the wider range of available goods, but those whose living standards fell were likely to be disillusioned with what is often called "barbarian capitalism." Similar trends were underway during this period in Lithuania and in Ukraine—the latter because its economy was closely interconnected with the Russian economy. For these reasons, we would expect that individual success in surviving harsh economic circumstance would condition one's outlook on market reforms. Indeed, the coefficients in Table 3 demonstrate that Soviets who felt that their own personal economic situation had improved or stayed the same were more inclined toward individualism than those who perceived a worsening situation. Similarly, respondents who were optimistic about future economic improvements were more likely to express support for individual responsibility. Also, those who rated communists very negatively were much more likely to place responsibility for social well-being with the individual than were those who rated communists more positively.

An unexpected result revealed by Table 3 is the weak statistical effect for concerns about national economic conditions.[3] This finding contradicts those studies arguing that sociotropic concerns generally outweigh the impact of pocketbook concerns. The results obtained here, however, point to an important set of circumstances, relevant to contemporary Eastern European and post-Soviet societies, that condition the relative impact of pocketbook and sociotropic assessments. Because the economic deterioration in these countries has been so widespread and visible, a large majority perceive that the national economy has declined and will most likely decline even farther before it begins to improve again. Under these circumstances, similar to those of a depression in Western democracies, individual survival and opportunity become far more important to each person than concerns about what is happening at the national level. Indeed, by 1991 the perception that the Soviet national economy had worsened during the past year had become almost universal (92% of the survey respondents said it had worsened), thereby providing virtually no explanation for differences in attitudes toward the locus of responsibility. Despite slightly more variance in the 1992 public assessments of the national economic performance in these new post-Soviet countries, 85% of the combined respondents said that the national economy of their country had further deteriorated during the past year. Given such widespread pessimism about the national economy, it is understandable that differences in more immediate, personal concerns are predominant.

Including the personal economic situation variable in the multivariate analysis does more than just demonstrate the strong impact that short-run changes in the economy can have on broader economic orientations such as locus of responsibility. The addition of the personal economic variable also changes the interpretation that can be given to the social structural variables included in the equation. Variables such as age, education, and place of residence (urban/rural) have previously been used by some writers as indicators of longer-term change, such as socialization or modernization effects (see e.g., Hahn 1991; Lapidus 1989). However, each of these indicators can also be expected to be correlated with short-term fluctuations in economic conditions. For example, the quality of life among elderly people on fixed incomes will be influenced far more by rapid price increases than will the life-style of younger people. Having controlled for the confounding effects of recent economic change, the statistically significant social structural variables actually take on increased relevance as indicators of longer-term socialization effects and the impact of social change (e.g., modernization) on the development of individualistic responsibility.

Yet more revealing, however, is the substantial impact of anticommunist sentiment on locus of responsibility. Even without trying to impose a particular causal interpretation on this relationship, it is important because it demonstrates a substantial link between broader economic ideology and political evaluations. Support for the communist system and striving for individualism were clearly at odds. Indeed, a separate analysis for 1991 that went beyond Table 3 reveals that the highest levels of endorsing individualistic effort as the source of well-being occurred among those who were alienated from the all-union government while trusting the republic-level leadership. Clearly political orientations were among the most important factors affecting locus of responsibility. Moreover, the strong impact of anticommunist sentiment on locus of responsibility demonstrates that materialistic benefits were not the only motivations underlying broader economic orientations.

SUPPORT FOR REFORM: NATIONALITY DIFFERENCES

The inability to confirm the results presented by Finifter and Mickiewicz also extends to their analysis of Soviet attitudes toward political reform, especially as political reform relates to nationality differences. For example, Finifter and Mickiewicz conclude that "Russians, Ukrainians and Balts did not differ very much from each other in the degree of support for

TABLE 4

Rapid Versus Slow Reform by Nationality

RAPID/SLOW REFORM	RUSSIANS IN RUSSIA 1990	1991	1992	UKRAINIANS IN UKRAINE 1990	1991	1992	LITHUANIANS IN LITHUANIA 1990	1991	1992
Pro–rapid reform (%)	43	43	37	39	46	37	86	75	45
Neutral (%)	16	19	18	20	17	20	9	15	27
Pro–slower reform (%)	41	38	45	41	37	43	5	10	28
Number of cases	397	979	821	284	495	507	340	347	242
Means	5.16	5.29	4.84	4.99	5.33	4.87	7.65	6.80	5.41

Source: University of Iowa's Post-Soviet Citizen Surveys.
Note: Means are calculated for a scale that ranges from 1 to 9, with high values indicating stronger pro–reform orientations.

political change" (1992, 862). Yet recent scholarship reveals that major forces for reform came from social and regional groups mobilized on the basis of nationalist symbols, grievances, and demands (Clemens 1991; Lapidus 1992; Motyl 1992).

Our hypothesis, which arises from our earlier work on identities (Miller, Hesli, and Reisinger 1993), is that nationality has a profoundly different impact depending upon the issue or policy area under consideration. More specifically, attitudes that relate directly to political reform can be expected to be more strongly influenced by nationality differences, while orientations related to economic reform and responsibility are less subject to nationality differences. These expectations arise from an understanding that the notion of political reform at the end of the Gorbachev era had clear anti-Soviet-state underpinnings and that support for political change often implied support for a restructuring of the Soviet system of authority. Thus political change became a rallying point for the multiple nationalist movements operating in the period from 1989 through 1991. Economic reform, however, presumably was less closely tied to nationalist aspirations, and a broader range of orientations toward state versus private ownership and economic responsibility should be apparent within national groupings.

To test the hypothesis that the relative impact of nationality depends upon the issue area, we included nationality as one of the independent variables predicting the locus of responsibility and the political change indices. Table 4 reports the distribution of a multiple-item index constructed by drawing from our surveys those items most similar to those employed by Finifter and Mickiewicz. The items include three questions from an *agree/disagree* battery:

1. It is better to live in an orderly society than to allow people so much freedom that they become disruptive.
2. These days Stalin is not given adequate credit for building of socialism.
3. Political reform in this country is moving too rapidly.

Also included in the index is a rating given of the Communist party, according to the following instructions:

> Now we would like to get your feelings toward certain groups using a five-point scale, where 5 indicates a very positive view and 1 a very negative view. You may use any number between 1 and 5 to tell me how favorable or unfavorable your feelings are for each group. If you have difficulty answering, just tell me and we will go on to the next one.

These items were selected because of their internal theoretical consistency: each item represents an important aspect of the reform process.[4] Item 1 corresponds most directly with Finifter and Mickiewicz item q22d, which makes reference to the trade-off between freedom and order. Item 3 corresponds to Finifter and Mickiewicz item q18, which makes specific reference to the pace of change. Item 2 is included because the repudiation of Stalin played a critical role in early efforts to legitimize the new reform effort. The reference to the Communist party is included because the focus of much of the reform effort was the intransigence, corruption, and lack of responsiveness of the Communist party.

It is noteworthy that both Russians and Ukrainians were generally less supportive of rapid change than were Lithuanians. Many of the explanations that have been offered in the literature for the relatively weaker manifestations of *perestroika*-era nationalism in Ukraine as compared with the Baltic countries, may account as well for the hesitancy of Ukrainians whole-heartedly to support rapid reform (see Armstrong 1990; Laitin 1992; Motyl 1987; and Nahylo 1991). Ukrainians from the eastern and southern regions exhibited levels of assimilation (russification) unmatched anywhere else in the Union, with the possible exception of Belorus. Adoption of Russian as their native language by Ukrainians in these regions was extremely high, while in West Ukraine (which was incorporated late into the Soviet Union during World War II and where most of the nationalists tended to reside) knowledge of Russian was much lower. Thus the majority of the population of Ukraine (found in the southern and eastern regions) were uncomfortable with the demands for radical change emanating from outside of their own region.

As of 1992, however, the republics were independent and formally in control of their own destiny. Following the collapse of the Soviet Union, we might expect the notion of political change to take on a different meaning—a shift that is suggested by the 1992 data in Table 4. In general, the data reveal a decline, as of 1992, in support for rapid reform (across all three national groups). Nevertheless, even after taking the general trend of declining support for political change into consideration, Lithuanians continued to exhibit stronger pro-reform orientations, thereby contradicting the Finifter and Mickiewicz findings.

Our longitudinal data also fail to support the Finifter and Mickiewicz findings regarding the impact of context on the attitudes of Russians residing in non-Russian republics. At the outset of the 1990s, approximately 24 million Russians lived in the non-Russian republics. This substantial presence of Russian minorities in a variety of places where the culture and climate of opinion is very different from that found in Russia raises the possibility that the local context may have influenced the attitudes of the nonindigenous Russians in a way that would produce preferences that differ from those expressed by Russians in Russia. Indeed Finifter and Mickiewicz contend that their data support the general argument that "the political attitudes of individuals who change their residential locations tend to be influenced by the political and social context into which they move; such individuals tend to change their attitudes in the direction of conformity with their new neighbors" (1992, 864). The evidence they provide in support of this contention is that Russians living in Kazakhstan, Uzbekistan, and Ukraine were very similar to the indigenous nationalities in terms of their attitudes toward political change (see p. 863). Likewise, they conclude that "the Baltic peoples . . . do not differ significantly from Russians in Estonia on support for political change" (p. 864).

Given that we have already demonstrated a similarity between Russians and Ukrainians in 1990 and 1991 in outlooks on the pace of change, the most telling comparisons involve a focus on differences between Russians and Lithuanians. As indicated in Table 4, indigenous Russian and indigenous Lithuanian attitudes toward change do differ, so that it is possible to test a hypothesis that predicts a similarity of attitudes between Russians living in Lithuania and Lithuanian nationals. Among Russians living in Lithuania in 1992, the group mean on the reform index is 4.96. This indicates that Russians living outside of Russia are very similar (as a group) to those living inside of Russia (see Table 4) but are different from Lithuanians. In fact, across all three years, the reform attitudes of Russians living in Lithuania are more similar to those of Russians in Russia than they are to those of titular Lithuanians.

The finding that Russians in Lithuania fail to share the attitudes of indigenous Lithuanians makes a good deal of sense. After all, Lithuanians on the whole were relatively negative toward Russia and Russians, sentiments that stem from their forced entry into the Soviet Union after World War II. Moreover, with independence came increased discussion about exclusionary policies that would make voting and the holding of public jobs dependent on the ability to speak Lithuanian—changes that could limit the opportunities for people who speak only Russian. Russians in Lithuania, therefore, would have good reasons to support slower change and to be concerned about possible social disorder, because they, as a group, would be disadvantaged by rapid change.

TABLE 5

Regression Equation Predicting Support for Political Reform

INDEPENDENT VARIABLES	1991 b	1991 BETA	1992 b	1992 BETA
Life satisfaction[a]	−.298** (.029)	−.070	.161* (.030)	.043
Age	−.058** (.001)	−.276	−.053** (.002)	−.259
Education	.279** (.013)	.160	.180** (.014)	.106
Gender[b]	.415** (.047)	.061	.446* (.051)	.069
Rural residence	−.755** (.056)	−.096	.021 (.061)	.003
Income	.000 (.000)	.011	.000 (.000)	.071
Ethnicity				
Ukrainian	.124 (.055)	.016	.195 (.058)	.027
Lithuanian	2.74** (.168)	.112	.619* (.180)	.037
Constant	11.41		9.57	
Adjusted R^2	.15		.11	
Number of cases	2,188		1,936	

Source: University of Iowa's Post-Soviet Citizen Surveys.
Note: The pro-reform index ranges from 1 to 17, with high values indicating greater support for rapid reform. Standard errors are in parentheses.
[a] Satisfied = high.
[b] Male = high.
*$p < .01$.
**$p < .001$.

In order to assess the impact of nationality on support for reform relative to other potential social cleavages in post-Soviet society, we included nationality and a number of other demographic characteristics in a multiple regression predicting pro-reform attitudes. The particular demographic predictors included in the equations were selected so as to produce a multivariate analysis similar to the one presented by Finifter and Mickiewicz (1992, tbl. 8). Our analysis confirms the Finifter and Mickiewicz findings for a number of the predictor variables, particularly the effects for age, education, and gender on attitudes toward political change (see Table 5). The propensity of younger people to be relatively more supportive of political change is apparent in both 1991 and 1992. These age differences may reflect generational socialization effects, or they may indicate that young people are better able to withstand the traumas of the times and are less negatively impinged on by dramatic changes in the system. Similarly, the more highly educated respondents were consistently more reform-oriented than their less-well-educated counterparts. We agree with the Finifter and Mickiewicz explanation that the principle beneficiaries of the recent political changes have more often been the better-educated sectors of Soviet and post-Soviet society (1992, 866). The results reported in Table 5 also confirm that women have been uncomfortable with the pace and direction of political change. Finifter and Mickiewicz interpret this finding as meaning that women are generally more conservative than men (p. 867). However, as we have previously argued, orientations toward reform are not necessarily a reflection of women being more conservative but rather of women perceiving the changes that are currently taking place as having a disproportionately negative impact on them (see Hesli and Miller 1993).

The rest of the multivariate analysis fails to support the Finifter and Mickiewicz findings. First, it is noteworthy that the power of the demographic variables to explain the variance in pro–reform attitudes drops over the period.[5] The fact that these same variables could explain less variance in pro–reform attitudes in 1992 indicates that political change took on a different meaning after 1991. The decline in explained variance over this period may suggest that socialization and modernization effects were more important predictors of pro–reform attitudes in the early phases of the Gorbachev period but that other factors (presumably more current political and economic forces cutting across demographic cleavages) had greater impact at the end of the Gorbachev era.

Other contradictions involve the significant impact of Lithuanian nationality and the relationship with urban/rural residence in 1991. The Lithuanian case provides support for the hypothesis that even with controls for other demographic characteristics, Lithuanians expressed more support for political change than did Russians or Ukrainians. In addition, the magnitude of the coefficient associated with Lithuanian nationality is greater in Table 5 than in Table 3, indicating a more powerful impact of nationality differences on political rather than economic reform orientations. Place of residence (either urban or rural) also had an independent and significant impact on orientations toward reform in 1991. The direction of the relationship in 1991 indicates that people living in the rural areas were less comfortable with rapid reform than were their urban counterparts. It is noteworthy that the rural residence effect is not operative in 1992. If urban/rural differences can be explained by a simple reference to "greater conservatism in rural areas" (Finifter and Mickiewicz 1992, 868), then rural respondents in 1992 should also be more conservative. A full-fledged explanation of observed differences is beyond our present scope, but let it be said that the particular circumstances of the times must be taken into account when offering explanations for anti–reform orientations. Rural people cannot in all cases be assumed to be conservative and traditional. (For more on this argument, see Turner et al. 1993).

RELATIONSHIP BETWEEN SUPPORT FOR POLITICAL AND ECONOMIC REFORM

The most telling discrepancy between the Finifter and Mickiewicz findings and our replication, however, does not involve these demographic relationships. Rather, the most important divergence in the two sets of findings involves the relationship between support for political change and attitudes toward the locus of responsibility for economic well-being. Finifter and Mickiewicz report that preferences on political change and broad economic orientations reflect two separate independent dimensions of Soviet public opinion.[6] The implication of this multidimensionality of attitudes, according to Finifter and Mickiewicz, is that economic and political change could occur independently of each other and take very different directions, at least if public preferences have a strong impact on changes. Such a finding is not only unexpected but certainly contradicts much of the broader literature on political economy, as well as the suggestions of some analysts of economic change in communist states (e.g., Mason 1992).

To reexamine Finifter and Mickiewicz's finding of no significant relationship between political and economic orientations, we employ data from our 1991 and 1992 surveys. The results resoundingly disconfirm the Finifter and Mickiewicz conclusion that two independent dimensions underlie these attitudes. Our data reveal a strong positive correlation between support for political reform and preference for individualism and a market economy (the Pearson correlation coefficients are .431 for 1991 and .453 for 1992).

However, given that similar demographic variables influence both sets of attitudes, the correlations might be spurious. A more stringent test of the interrelationship of political and economic orientations would be to examine that interrelationship while controlling for demographic correlations. Because correlations do not reveal the direction of causality, we face the question whether the measure of political reformism or the measure of economic reformism should serve as the dependent variable. Others' recent analyses suggest, however, that political orientations have a stronger impact on economic outlooks than vice versa (Duch 1993; Gibson 1993). We therefore employ the locus-of-responsibility measure as the dependent variable in a multivariate regression. The results of this regression are shown in Table 6.

In both years, a strong positive association exists between support for political reform and a favorable view of individual initiative even after controlling for various demographic characteristics and the signifi-

TABLE 6

Regression Testing Hypothesized Relationship between Support for Political Reform and Locus of Responsibility

INDEPENDENT VARIABLES	1991 b	1991 BETA	1992 b	1992 BETA
Support for political reform[a]	.27**	.32	.29**	.33
	(.02)		(.02)	
Personal finances[b]				
Past	.32**	.07	.61**	.15
	(.05)		(.05)	
Future	.44**	.11	.42**	.11
	(.05)		(.05)	
Life satisfaction[c]	.15*	.05	.10	.03
	(.07)		(.07)	
Age	−.04**	−.21	−.03**	−.18
	(.00)		(.00)	
Education	.16**	.08	.08*	.05
	(.04)		(.03)	
Gender[d]	.53**	.10	.52**	.09
	(.10)		(.11)	
Urban/rural	−.17	−.02	−.09	−.01
	(.14)		(.14)	
Income	.00*	.05	.00**	.09
	(.00)		(.00)	
Ethnicity				
Ukrainian	.12	.02	−.20	−.03
	(.13)		(.13)	
Lithuanian	.67	.03	.45	.02
	(.39)		(.40)	
Constant	3.92		3.83	
Adjusted R^2	.28		.33	
Number of cases	2,128		1,938	

Source: University of Iowa's Post-Soviet Citizen Surveys.
Note: Locus-of-responsibility and pro–reform indices are defined in Tables 3 and 5, respectively. Standard errors are in parentheses.
*$p < .01$.
**$p < .001$.
[a]Pro–reform = high.
[b]Better off = high.
[c]Satisfied = high.
[d]Male = high.

cant economic (retrospective and prospective personal financial situation) and life satisfaction variables employed in Table 3. Moreover, adding the measure of support for political reform improves the explanatory power of both years' models beyond that obtained in Table 3.

The results of testing this model are largely the same for the 1991 and the 1992 data. One difference between the two years deserves note. Even after adding the political reform measure to the equations, the question asking whether the economic situation of the respondent's family had improved, stayed the same, or became worse over the past year had twice as powerful an impact in 1992 as in 1991, whereas the impact of the prospective measure remains unchanged. The strong positive coefficient for assessments of one's financial situation, particularly the retrospective assessments in 1992, thus confirms the results reported earlier in Table 3.

The picture that our data present seems clear. Mass orientations toward economic and political reform are not independent of each other: they do not oppose but rather reinforce one another in the former Soviet Union. Those most accepting of the former Soviet regime's political institutions and norms look least favorably on marked norms and institutions, and vice versa. This relationship holds true for both years of our surveys and even when we control for such key societal cleavages as age and level of education.

CONCLUSION

How do we explain the discrepancies between our findings and those reported by Finifter and Mickiewicz? In part Finifter and Mickiewicz themselves provide the answer, as they acknowledge that "single items are generally less valid than multi-item indicators" (1992, 861). Our analysis certainly does suggest that there were some problems with their Q22a as a single-item measure of attitudes toward the economic system. Whether these were problems of translation, question clarity, or data entry errors we do not know. To a great extent the discrepancies revealed by our analysis focus on that single item. Thus the validity of the entire data set is not necessarily called into question. (For more on concerns about the validity of the earliest surveys coming out of the Soviet Union, see Miller, Reisinger, and Hesli 1993.) Of course, the rapid change occurring in the Soviet Union during the Gorbachev era could also have contributed to the differences between our results and those reported by Finifter and Mickiewicz. The longitudinal data presented here, especially in Table 4, reveal the significant shifts in attitudes toward political reform that were taking place during the last days of the Soviet Union. It is possible, therefore, that some of the discrepancies could be accounted for by attitude volatility rather than limitations in the data.

Regardless of the discrepancies between the two sets of data, we should not lose track of the fact that on a number of points the various data are in agreement. Moreover, such data, particularly the longitudinal data, provide an empirical basis for verifying hypotheses regarding life and politics in the former Soviet societies. Clearly this is a tremendous improvement over what could be done in an earlier period when the collection of such data was not permitted.

The longitudinal data, as well as the regressions presented in Table 6, demonstrate that Soviet public attitudes on economic and political reform were more interconnected and consistent than Finifter and Mickiewicz suggest (1992, 869). Individuals who preferred political reforms of a democratic nature also favored a decreased role for the state as guarantor of economic well-being. Being a political reformist, therefore, does imply an abandonment of the socialist system in favor of increased individual responsibility even when it comes to the social welfare of citizens. In short, *perestroika,* which included policies that presumably promoted both political democracy and a shift toward decentralization, was a reflection of public preferences.

In addition, the longitudinal data reveal significant variation, both cross-sectionally and across time, in support for democratic and economic reform in the Soviet and post-Soviet societies. The analysis demonstrates that the explanations for this variation in public support for reform are complex and multifaceted. The self-interested pursuit of material benefits suggested by Finifter and Mickiewicz (1992, 870), is no doubt one factor accounting for the variation in support of reform. However, socialization, political ideology, and the public's response to current economic and political change may be even more important for understanding variation in support or opposition to continued reform in the post-Soviet societies. All of this, of course, leaves us with a challenging agenda for future research.

APPENDIX

The goal of the University of Iowa's Post-Soviet Citizen Surveys (PSCS) conducted in 1990, 1991, and 1992 was to interview a representative sample of adult citizens living in three Soviet republics: Lithuania, Ukraine, and the portion of Russia west of the Ural Mountains. The particular republics were selected on the basis of size of population, economic importance within the Soviet Union, and political relevance. Russia and Ukraine are the republics with the largest populations and highest gross national products among the 15 republics of the former Soviet Union. Although Lithuania accounted for less than 2% of the Soviet population, it was politically very important because, being among the Baltics, it represented the early moves toward secession from the Soviet Union. Primary funding for the surveys was provided by National Science Foundation Grants SES 90-23974 and SGER 90-09698. Soviet support for the project was provided by the Soviet Peace Fund, the Soviet Peace Committee, and the Soviet Political Science Association.

The Sample

The sample size for the study was 1,800 completed interviews in 1990, 3,000 in 1991, and 2,700 in 1992. The study design called for proper representation of the target population in each of the three republics. The total number of interviews in 1990 was divided equally among the republics, 600 in each. In 1991, the target sample was divided as follows: 1,400 in Russia, 1,000 in Ukraine, and 600 in Lithuania. The 1992 sample was comprised of 1,300 interviews in Russia, 900 in Ukraine, and 500 in Lithuania.

The sample approach used for selecting respondents was a four-stage stratified sample. At the initial stage, each republic was divided into strata reflecting the extent of urbanization, geographic region, the distribution of nationalities, and population density. These criteria produced eight strata for Ukraine and Russia and four strata for Lithuania. The second stage of the sample involved listing the places that fell into each strata and selecting, with probabilities proportionate to size, a number of *primary sampling units* from each strata. The 1990 sample, because of limited funds, employed 40 primary sampling units, whereas the 1991 and 1992 studies had 81 primary sampling units. The third stage of the sample involved enumerating all the voting districts for each of the primary sampling units. Between 4 and 23 voting districts were randomly selected for each primary sampling unit depending on the size of the place. The final stage of sampling involved selecting specific individuals from the voter lists—cross-referenced with residence records—for each of the selected voting districts. The procedure involved the calculation of a sampling interval *n* for each particular list, selecting a random number between 1 and *n* as the starting point and then selecting every *n*th person from that starting point. No substitution of respondents was allowed. The overall response rates in the studies were high by Western standards: 89.4%, 83.7%, and 87.4% 1990, 1991, and 1992, respectively. The response rates were virtually the same for each republic.

Questionnaire Construction

The survey instrument was designed through a series of meetings between the United States research team and a group of Soviet scholars: Andrei Melville, Alexander Nikitin, and Elena Bashkirova. The questionnaire was translated from English into Russian, Lithuanian, and Ukrainian and back-translated in the Soviet Union and in the United States using native-speaking Russians, Lithuanians, and Ukrainians. This translation-back-translation process was carried out a number of times until an agreement was reached on the particular translation to be employed. Prior to the final revisions, a pretest was conducted in Moscow, Kiev, and Vilnius. Neither the questionnaire nor the interviewers gave any indication that the survey was part of a collaborative project involving American researchers. The average interview took 38 minutes in 1990 and 45 minutes in 1991 and 1992. All interviews were conducted from 18 May to 21 June 1990, from 20 May to 25 June 1991, and in late June 1992. All questionnaires were later transported to the United States, where they were used to code open-ended questions and to correct any data entry errors.

Interviewing

The interviewers were recruited and initially trained by staff from the USSR Academy of Sciences, Institute of Sociology in Moscow, under the supervision of Elena Bashkirova. In addition, the American investigators provided further interviewer training sessions prior to the start of each study period. During the interviewing period, the American scholars traveled to the various sample points to supervise the data collection and to be certain that appropriate interviewing procedures were being followed.

Our experience of having conducted three waves of interviewing in the republics of the former Soviet Union has served to reinforce the need to take standard precautions aimed at assuring valid and reliable data. In addition to the usual concerns about sampling, proper translation of questions, and the appropriate training of interviewers, we feel that any researcher conducting survey research in the former Soviet Union must spend time in the field supervising the data collection, as well as verifying the quality of the interviewing and the accuracy of the recorded data. In addition, it is incumbent on the researcher to retrieve and archive the completed questionnaires so they can be used for correcting data entry errors. Finally, as a contribution to the advancement of the profession and as a means of checking the veracity of published results, all data used in scholarly work should be made available to others for secondary analysis through the ICPSR. The authors may be contacted for a more detailed description of the surveys.

Questionnaire Items Used in the Locus-of-Responsibility Index

1. Some people say the government of Russia [Ukraine, Lithuania] should guarantee everyone work and a high standard of living, others argue that every person should look after himself. On this card is a scale from 1 to 5, where 1 signifies that the government guarantees everyone work and 5 that every person should look after himself. Which position corresponds to your view?
2. There should be a mechanism regulating income such that no one earns very much more than others. (*agree/disagree*)
3. Speaking only of Russia [Ukraine, Lithuania], what type of property would be best: private property, collective property or only state property?

4. Some people think that certain groups in society have too much influence on life and politics in Russia [Ukraine, Lithuania] and that other groups don't have as much as they deserve. Do you think "Businessmen" have too much, too little or the right amount of influence?

Notes

We wish to thank Chia-hsing Lu and Andy Peebler for assistance in the data analysis and Peggy Swails for her secretarial assistance.

1. The data from the 1990 survey is now publicly available through the ICPSR. The 1991 and 1992 survey data will be available for reanalysis by the end of 1994.

2. The Cronbach alpha reliability coefficients for a four-item scale were .68 and .63 for 1991 and 1992 respectively.

3. For statistical significance, we are restricting the probability of type I error to less than .01. The item, however, had relatively little variance, so that some evidence of a relationship between evaluations of the national economy and locus of responsibility does exist.

4. Cronbach's alpha for the proreform index in each of the three former republics for 1991 and 1992 is as follows:

Year	Russia	Ukraine	Lithuania
1991	.635	.635	.448
1992	.656	.478	.407

5. The regression analysis for 1990 is not presented in Table 5. All demographic variables, and the single attitudinal indicator have the same directional impact as in 1991 and 1992. The major difference is the fact that the set of variables explains a larger portion of the variance in 1990 as compared with the later years. We excluded 1990 from the regression analysis because the proreform index can be expanded to a 17-point scale if we rely only on the 1991 and 1992 data. In these years, the options given to respondents on the *agree/disagree* battery employed five options from *strongly agree* to *strongly disagree*. In 1990, the options were limited to *agree, neutral,* or *disagree.*

6. Finifter and Mickiewicz actually report a slight negative correlation between locus of responsibility and political reform (1992, 861). But on the very same page they also report that the results of a factor analysis demonstrate that Q22a, the single item measuring locus of responsibility "is orthogonal to the other items."

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American Political Science Review Vol. 88, No. 2 June 1994

BAYESIAN INFERENCE FOR COMPARATIVE RESEARCH

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Regression analysis in comparative research suffers from two distinct problems of statistical inference. First, because the data constitute all the available observations from a population, conventional inference based on the long-run behavior of a repeatable data mechanism is not appropriate. Second, the small and collinear data sets of comparative research yield imprecise estimates of the effects of explanatory variables. We describe a Bayesian approach to statistical inference that provides a unified solution to these two problems. This approach is illustrated in a comparative analysis of unionization.

A widely used tool in quantitative comparative politics is the linear regression model, estimating relationships between institutional and economic variables observed for a set of countries constituting some population of interest. A brief survey of recent comparative research finds no shortage of work of this type. For instance, Remmer (1991) identifies economic crisis as a source of electoral volatility in Latin American countries in the 1980s; Robertson (1990) presents evidence that institutions moderating the transaction costs of negotiations between capital and labor reduce industrial conflict; and Golden (1993) finds that union wage moderation in industrialized democracies results from coordinated collective bargaining.

Although the methodological advantages are well documented (Jackman 1985), we identify two distinct problems of statistical inference endemic to this type of quantitative comparative research. First, because the data constitute all the available observations from a population, conventional inference based on the long-run behavior of some repeatable data mechanism is not appropriate. Second, because data sets tend to be small and collinear, the effects of explanatory variables are estimated imprecisely. In most applications the first problem is generally ignored and the second is solved by imposing restrictions on the regression model to obtain "sensible" results.

The Bayesian model of statistical inference provides a unified solution to these two distinct problems of quantitative comparative research. Two features of Bayesian inference are important in this context. First, probability is conceived subjectively as characterizing a researcher's uncertainty about the parameters of a statistical model. This subjective probability concept seems particularly useful in comparative settings, where the data are typically convenience samples not generated by a known probability mechanism such as random sampling. Second, Bayesian inference allows the inclusion of other information, in addition to the quantitative sample information. Again, this seems useful in the comparative context where rich historical material is commonly available, sparking ideas for researchers, but formally discarded in the final analysis. In the Bayesian approach, this historical material can be formally incorporated into the analysis.

We begin by detailing why the nonstochastic and weak data typical of comparative research are problematic for conventional linear regression analysis. We next introduce some ideas about Bayesian inference and describe Bayesian regression analysis. These ideas are applied to a comparative analysis of unionization—the subject of a recent exchange in the *American Political Science Review* between Michael Wallerstein and John Stephens (Stephens and Wallerstein 1991; Wallerstein 1989). The Bayesian regressions we present are supplemented with a sensitivity analysis that investigates how our conclusions depend upon the sample data and our prior beliefs.

Although we survey some ideas in Bayesian statistics, a full introduction to the Bayesian approach to statistical inference is beyond our scope here. (Good book-length treatments are Pollard 1986, Lee 1989 and, at a slightly higher level, Press 1989.) Our intention here is to spotlight problematic but ignored issues of statistical inference in a common area of application. The Bayesian alternative that we present represents just one way forward to address these inferential problems.

TWO FEATURES OF COMPARATIVE DATA ARE PROBLEMATIC

A common design in comparative research generates data with two important characteristics: (1) the data constitute all the available observations from a population; and (2) the data, because of small sample size and collinearity, tend not to be very informative about the statistical parameters being estimated. Although our focus on comparative research is stimulated by the preponderance of studies with data of this type, our arguments are generalizable to other areas using data with these characteristics. For example, studies comparing the American states could well be another area of application (e.g., Barrilleaux and Miller 1988; Erikson, McIver, and Wright 1987).

Nonstochastic Data

Unlike analysts of survey research data or experimenters who randomly assign subjects to treatments, comparativists often collect all the available observations from some population of interest. For example, populations including "advanced industrial societies" (Wallerstein 1989), "contemporary democratic political systems" (Powell 1982, 2), and the "affluent market-oriented democracies" (Swank 1988, 1121) have all been studied by comparative researchers. In contrast to experimental or social survey data, comparative data are not generated by a repeatable data mechanism. Repeated applications of survey or experimental designs, on the other hand, yield new data sets with more information about the process under study. In comparative research, once all the data are collected from a population, further applications of the data collection process do not yield more information. Data of this type, generated by a nonrepeatable and unknown probability process, are described by Freedman and Lane (1983) as "nonstochastic." We adopt their terminology here.

To see why nonstochastic data create problems for conventional statistical inference, we briefly review the concept of frequentist probability. This idea is the workhorse of conventional, or frequentist, statistics. In 1837, Denis Poisson defined probability as the limiting distribution of a long-run relative frequency. If, for example, a coin is tossed n (a very large number) times and shows m heads, we can write the prob(heads) $\equiv p = m/n$, assuming that m/n coverges. In this case, the probability of heads, p, is the proportion of heads that will be observed when n grows to infinity. The probability, p, like size or weight, describes an objective characteristic of the coin (Barnett 1982, chap. 3; Leamer 1978, chap. 2).

Frequentist statistical inference assumes that data are generated by a repeatable mechanism such as the coin flip. Von Mises provides the paradigmatic statement of this view: "In order to apply the theory of probability we must have a practically unlimited sequence of uniform observations" (quoted in Barnett 1982, 76). A sample observation is thus just one possible result from many possible draws from a probability distribution. In practice, these draws are implemented by probability sampling (as in a social survey) or random assignment (in an experiment). Frequentist inference makes conclusions about a parameter (perhaps a mean or a regression coefficient) obtained from a sampling or assignment process, as if that process was repeated a large number of times. For example, a random sample survey of American adults may indicate that mean income in the United States is $35,000. Assuming (rather implausibly) that income is normally distributed, we could estimate a 90% confidence interval for our sample mean, perhaps [$15,000, $55,000] for a modestly sized sample. Using conventional frequentist inference we can conclude that intervals like the one calculated would cover the true (population) mean income 90% of the time for repeated applications of the sampling procedure. The "repeated sampling" inference tells us neither whether the population mean lies within the estimated interval nor even with what probability the mean lies in the interval. Specifically, the frequentist inference does not entitle us to claim there is a 90% chance that the true mean falls within the estimated interval. The only available conclusion concerns the long-run behavior of a statistic—in this case, the 90% confidence interval.

While frequentist inferences from survey data allow conclusions about probability sampling processes, nonstochastic data create problems for frequentist inference because they are not generated by a repeatable mechanism. Frequentist inference is simply unrealistic given the manner of data collection. Comparative researchers sometimes show an uneasy awareness of this problem. In their analysis of advanced industrial democracies, Lange and Garrett frankly report that they "adhere to traditional standards [i.e., significance tests] while remaining unsure of their applicability" (1987, 268). In a similar vein, Weede endorses the use of t-statistics for testing hypotheses in his study of welfare state size in industrial democracies, not only for methodological reasons but also because "it is almost universal practice in econometrics and the social sciences" (1986, 518). More confusion is revealed in Gorin's comparative study of inequality that "cannot utilize probability theory to ascertain the level of significance" for want of "random samples"; he goes on to note that asterisks indicate a ".05 level of significance" (1980, 3:153).

Comparative researchers' discomfort with frequentist inference is well founded because *frequentist inference is inapplicable to the nonstochastic setting.* It is simply not relevant for the problem at hand to think of observations as draws from a random process when further realizations are impossible in practice and lack meaning even as abstract propositions. In short, frequentist inference answers a question that comparative researchers are not typically asking (see also Freedman and Lane 1983, 189).

Two objections might be raised at this point. One challenge might be that statistical inference is unnecessary in the nonstochastic setting because all the available information is collected. This position is certainly valid, but it still commits the researcher to a substantive theory of how the data were generated. In particular, if there is no uncertainty associated with the data and if statistical inference is expendable, the researcher is effectively claiming that things could not have been different, that the data were generated by a completely deterministic process. This suggests a thought experiment: If we could set in motion, once again, the historical conditions that gave rise to the data, would the ensuing process generate a data set identical to the one actually obtained? Commitment to this type of determinism is the cost of abandoning statistical inference (Berk, Western, and Weiss 1993).

The second objection holds that although the data mechanism is not repeatable, it can be treated as if it were. In this view, the social world randomly draws observations from a set of all possible observations, or a "superpopulation" (Cochran 1953, 169). The data are one realization of all possible data sets that might have been collected. Or as one reviewer put it, "each country's history is one draw of a distribution of possible histories." While this assumption avoids a deterministic theory of the data process, it is highly speculative compared to positive knowledge about a sampling procedure or, in Fisher's uncompromising phrase, "the physical act of randomization" (quoted in Freedman and Lane 1983, 197). Even more troubling, however, is the conclusion that conventional inference allows in this instance. Take the example of a confidence interval for a mean where we can conclude that under repeated realizations (which are acknowledged to be impossible), the interval would cover the true mean 90% of the time. We have no way of knowing whether the current interval is one of the fortunate 90% and no possibility of further replications. (Our focus here has been on the confidence interval because this is the inferential tool on which we shall rely in our application and the one that implicitly underlies classical hypothesis tests. Similar criticisms of classical hypothesis testing and other aspects of frequentists inference are reviewed in Barnett 1982, 180–88).

Weak Data

The problem of statistical inference in comparative research is exacerbated by the weakness of comparative data. These data are weak in the sense that they provide little information about parameters of statistical models. The uninformative character of the data is reflected in large standard errors and consequently large *p*-values.

Weak comparative data have two distinct causes. First, the sample sizes characteristic of comparative research are small in relation to the number of parameters being estimated. While comparativists frequently make do with less than a hundred data points, survey researchers frequently have more than a thousand. For example, comparative researchers have defined the population of advanced capitalist democracies to include between 15 and 21 countries (Lange and Garrett 1985; Wallerstein 1989). Sometimes, even where samples sizes are fairly large, a relatively large number of parameters must be estimated (e.g., Williams 1991). Ideally, if more data could be collected this would be one solution to the uninformative data problem. As the sample size increases in relation to the number of parameters, the estimated variances of the regression coefficients would tend to decline.

Collinearity is the second cause of weak comparative data. In a comparative setting, collinearity is common because the explanatory variables are themselves often causally related. When explanatory variables are collinear, they carry little independent information about the various regression coefficients. A regression on collinear predictors can yield estimates with unexpected signs and large standard errors.

To foreshadow some Bayesian ideas, collinearity provides no statistical difficulties except in very extreme instances (of exact or near-exact linear dependencies) which are seldom encountered in practice. The least squares estimators retain their properties, and statistical inference can proceed as normal. Why, then, is collinearity regarded as problematic? The difficulties caused by collinearity—large standard errors and unexpected signs for regression coefficients—are only problems with respect to prior expectations about the signs and variances of the coefficients (Leamer 1978, 170). As Leamer argues, collinearity is thus not a statistical problem but a problem of the interpretation of multidimensional evidence (pp. 170–73). As a tool of interpretation, the least squares estimator in the presence of collinearity does not allow us to distinguish information about one coefficient from information about another. Instead, the least squares estimator is informative about linear combinations of coefficients. The introduction of nonsample information allows sample information to be allocated among the coefficients according to substantive criteria.

A more familiar approach to collinearity involves constraining coefficients to zero (excluding them from the analysis). This is an informal way of introducing information to obtain sharper estimates of other coefficients. The researcher is implicitly saying, "To obtain a sensible estimate for the effect of x_1, I must estimate the model on the basis of the substantive claim that x_2 had no effect at all." The substantive claim that x_2 has no effect allows the weight of the sample evidence to be allocated in favor of x_1. Here, of course, a "sensible" estimate of the regression coefficient for x_1 is based on (typically unstated) prior expectations. Dropping x_2 introduces known specification errors, and in part defeats the motivation for statistical control that led to multivariate techniques in the first place. (The papers in Granger 1990 on model specification provide an elaborate treatment of this issue from Bayesian and non-Bayesian perspectives in econometrics.)

In sum, the weak evidence of comparative research that results from small data sets and collinearity yields weak or fragile inferences that are highly sensitive to the model specification. This is the natural outcome of an analysis based on little evidence. The only solution to this problem is the introduction of more information. But as a practical matter, more information is frequently not available. Data have been collected from all the countries in the sample. The growing popularity of pooled cross-sectional times series designs suggest one remedy to the weak data problem (e.g., Alvarez, Lange, and Garrett 1991; Beck et al. 1993; Radcliff 1992; Swank 1992); but additional time points often provide little new information because the processes being studied show far more cross-national than longitudinal variation (e.g., Wallerstein 1989, 482). Institutions, for example, do not change much over time but show considerable cross-national variation. Furthermore, the collinearity problem is distinct from small sample size and can afflict the pooled cross-sectional design irrespective of the number of observations. However, while extra quantitative information is typically unavailable, large and substantively rich stores of qualitative information from comparative and historical studies are often present but not available in a form suitable for analysis. Bayesian procedures enable the weak quantitative information of comparative research to be pooled with the qualitative information to obtain sharper estimates of regression coefficients. Although we shall focus on situations in which the data are weak, Bayesian methods can also be applied (generally with less consequence) where the data are highly informative.

BAYESIAN INFERENCE

The Bayesian approach to statistical inference involves pooling nonsample (or "prior") information with sample data to formulate posterior subjective probability statements about the parameters of a statistical model. This description of Bayesian inference signals two aspects of the Bayesian approach that distinguish it from conventional inference. First, Bayesian inference is built upon a subjective probability concept. Second, Bayesian inference allows the introduction of prior information in addition to the sample information to make statistical inferences.

Subjective Probability

In contrast to the frequentist concept that refers to objective probability features of the world, subjective probability refers to a person's *degree of belief* in an uncertain event. Subjective probability is thus a personal statement of certainty or confidence, rather than a fact characterizing an object in the external world. Subjective probabilities share the same axioms as objective probabilities and are consistent with formal rules for rational decision making (Barnett 1982, chap. 3). The difference between subjective and objective probability resides in whether the probability statement refers to an individual's personal feeling of confidence, or whether it characterizes an objective feature of the world.

Prior Information in Regression

In Bayesian inference, researchers' subjective probability assessments of the parameters of a statistical model are pooled with the sample data to arrive at posterior probability statements about those parameters. The prior information is represented in a probability distribution. In the normal regression case that we are concerned with, this prior probability distribution can be summarized by prior means and variances for the regression coefficients. (Tanner 1993 considers more complicated applications.) The posterior probability statements express the researchers' degree of belief in the parameters, given the data and the prior, replacing conventional inference about the distribution of coefficient estimates in repeated sampling. We can begin to see how prior information is incorporated into a data analysis by first reviewing how two data sets might be pooled in a regression analysis.

Two data sets, (X_1, y_1) and (X_2, y_2), can be pooled for a regression analysis as follows:

$$\begin{bmatrix} y_1 \\ y_2 \end{bmatrix} = \begin{bmatrix} X_1 \\ X_2 \end{bmatrix} \beta + \begin{bmatrix} u_1 \\ u_2 \end{bmatrix}.$$

Following Leamer, if we write σ_1^2 and σ_2^2 for the two residual variances, the least squares estimate of β from the pooled data is

$$b_{pooled} = (\sigma_1^{-2}X_1'X_1 + \sigma_2^{-2}X_2'X_2)^{-1}(\sigma_1^{-2}X_1'X_1b_1 + \sigma_2^{-2}X_2'X_2b_2), \quad \textbf{(1)}$$

where $b_1 = (X_1'X_1)^{-1}X_1'y_1$, and $b_2 = (X_2'X_2)^{-1}X_2'y_2$ (1978, 76).

Although equation 1 may look a bit cumbersome, the pooled estimate for β is just a (matrix) weighted average of the two sets of least squares estimates obtained from each of the data sets.

Equation 1 is useful from the Bayesian point of view, because it suggests how sample and prior information can be combined to obtain posterior distributions for regression coefficients. The regression coefficients estimated from the first data set, b_1, can be replaced by a set of regression coefficients that the researcher believes a priori are most probable. These are the prior means, written b^*, of the prior probability distributions we have referred to. Note that the inverse of the first part of the first term in equation 1 is the estimated variance of the coefficients from the first data set. This variance can be substituted by the subjective Bayesian prior variance, V^*, for the coefficients. Uncertainty about the prior means is reflected in the specification of V^*. Larger variances for a prior mean imply greater uncertainty. Prior covariances—off-diagonal terms in the prior covariance matrix—can also be specified if beliefs about one coefficient depend on beliefs about others —say, if one is estimating main and interactive effects in a regression equation and if confidence in the interaction effects depends on confidence in the main effects (e.g., Lange and Garrett 1985). The prior covariances, expressing dependent beliefs about the regression parameters, thus do not directly relate to prior beliefs about relationships in the sample data. Because beliefs about the coefficients are generally independent, the prior covariance matrix for the regression coefficients is often specified to be diagonal.

Substituting the prior information into equation 1 provides an expression for the posterior mean based on a combination of the prior and sample information:

$$b = (V^{*-1} + \sigma^{-2}X'X)^{-1}(V^{*-1}b^* + \sigma^{-2}X'y) \quad \textbf{(2)}$$

where X and y designate the sample information (the subscripts are now redundant) and the residual variance, σ^2, is estimated from the sample data. (Note that when the expression for the sample parameter estimate is expanded in equation 1, the cross-product matrices in the second term cancel out, leaving $X'y$.) The variance of b is given by the first term of equation 2 and standard deviations for the posterior coefficients are the square roots of the diagonal elements of this matrix. An alternative route to these expressions for the posterior means and variances of the regression coefficients is to multiply a normal prior distribution by a normal likelihood for the sample data, treating the residual variance as known. This is an application of Bayes Theorem, and it yields a multivariate normal posterior distribution for the coefficients. The posterior means and variances of this multivariate distribution are identical to the expression we obtained in equation 2. The pooling exposition highlights, however, how subjective prior information can supplement the weak sample data common in comparative research.

Approaching the Bayesian regression problem in this way, the posterior is simply a pooled estimate based on the sample data and the "data" represented by the researcher's prior beliefs. Bringing prior information to bear on an inferential problem via Bayes Theorem involves the same procedure as if one were to collect more data: in both cases, the researcher pools the sample information with the prior distribution or additional data using equation 1 or equation 2.

The resolution of collinearity problems through the introduction of prior information is made clear by the expression for the posterior variance—the first term of equation 2. If prior means are set to zero, a diagonal prior covariance matrix will improve the conditioning of the explanatory variables, resulting in regression coefficients with smaller variances. This estimator is a special case of the generalized ridge estimator (Belsley 1991, 300), except that in the Bayesian case the ridge for the sample cross-products matrix is chosen according to substantive criteria rather than arbitrarily. Bayesian regression thus solves the problem of collinearity by using more information than ordinary least squares, generating smaller standard errors for the regression coefficients.

A convincing Bayesian analysis goes further than just calculating the posterior distributions. Because the two ingredients for Bayesian regression—the prior and the sample information—influence the final results, a thorough analysis should explore the sensitivity of these results to the prior and the data. Robust findings yield substantive conclusions that are insensitive to small changes in the sample and nonsample information. These ideas will be detailed below.

In sum, Bayesian regression analysis provides answers to the problems of comparative research that we have identified. First, subjective probability replaces the conventional concept, providing a more realistic basis for analyzing data collected without a repeatable data mechanism. Second, more information is brought to the analysis through the prior distribution. Weak data resulting from small samples and collinearity (common in comparative research) are bolstered by the prior, increasing the precision of the estimated regression coefficients. Finally, the robustness of Bayesian results can be explored through an analysis of the sensitivity of the posterior to the sample data and the prior information.

BAYESIAN REGRESSION ANALYSIS IN A MODEL OF UNION DENSITY

We illustrate Bayesian regression analysis in the context of a recent exchange between Wallerstein and John Stephens. Wallerstein and Stephens are concerned to explain cross-national variation in union density—the percentage of a work force that are union members. Wallerstein argues that the size of the civilian labor force is a key determinant of union density. Stephens claims that density depends on industrial concentration. These two predictors correlate at −.92 and Stephens writes that "because of multicollinearity, economic concentration and the size of the labor force could not be entered in the same equation" (Stephens and Wallerstein 1991, 945–46). Of course, as noted, no statistical argument prevents the inclusion of both predictors in a regression on union density. Stephens is simply observing that because of the correlation between labor force size and industrial concentration, the data will not be very informative about coefficients for either variable, and the results may be sharply at odds with prior expectations. The problem of collinearity results partly from incomplete data for economic concentration. Stephens uses information about logged gross domestic product to impute missing observations for 9 of the 20 countries in the sample, yet gross domestic product also correlates highly with labor-force size. For this analysis, we bracket the issue of missing data, although it should be noted, with Wallerstein, that Stephens's imputation strategy provides only weak information about economic concentration (pp. 949–50).

Two other features of the Stephens–Wallerstein data are also noteworthy. First, the sample is not generated by any known probability mechanism such as random sampling. The 20 countries comprising the sample were chosen because they experienced a continuous history of political democracy since World War II (Wallerstein 1989, 489). In the absence of some rather fanciful assumptions about probability features of the data-generating mechanism, the classical model of statistical inference is inappropriate. Second, the weak data problem that results from collinearity is compounded by the small sample that provides little statistical power. Stephens implicitly recognizes this problem in the analysis by using a relatively large level of statistical significance (.1). Here, then, is a classic example of weak data generated by an unknown probability mechanism in a

TABLE 1

Prior Means (Standard Deviations) and Their Substantive Interpretations for Bayesian Regression Analysis of Union Density

VARIABLE	PRIOR	SUBSTANTIVE INTERPRETATION
Wallerstein's priors		
Left government	.3 (.15)	One year of left-wing government increases union density by about 1 percentage point. A year of left-wing government may increase union density by as much as 2 percentage points of union density, but its effect is almost certainly not negative.
Logged labor-force size	−5 (2.5)	Doubling the size of the labor force would reduce union density about $\ln(2) \times 5 \approx 3.5$ percentage points. This increase in labor-force size may generate a union decline as big as 7 percentage points, but a growing labor force is unlikely to increase union density.
Economic concentration	0 (10^6)	The diffuse prior indicates that the researcher has no strong prior beliefs about the sign or magnitude of an effect. When the explanatory variables are uncorrelated, the diffuse prior yields posteriors that are approximately given by the sample data.
Stephens's priors		
Left government	.3 (.15)	Like Wallerstein's prior, one year of left-wing government increases union density by 1 percentage point.
Logged labor-force size	0 (10^6)	Diffuse prior.
Economic concentration	10 (5)	If economic concentration were to increase by 100% in relation to the United States, union density would increase by 10 percentage points. This increase in the concentration ratio may generate a density increase as large as 20 percentage points, but any increase in concentration is unlikely to decrease union density.

Note: Left government is measured by Wilensky's (1981) cumulative index of left-wing government; logged labor-force size is the natural log of the size (in thousands) of the dependent labor force in the year that union density is measured; and economic concentration is measured by the four-firm concentration ratio, in proportion to the United States.

comparative setting. Our data are taken from, and described by, Stephens (Stephens and Wallerstein 1991). Note that labor-force size is measured on the log scale, so that the coefficients express the changes in union density in response to percentage changes in labor-force size. Economic concentration is measured as a ratio to U.S. economic concentration, so that the coefficients express the change in union density as a result of a country's change in economic concentration in relation to the United States.

What light can Bayesian regression analysis shed on the Stephens–Wallerstein debate? From the Bayesian perspective, the source of the controversy between Stephens and Wallerstein is the data that are not strong enough to allow a convincing resolution one way or another. Both researchers informally enlist more information to support their arguments. In Wallerstein's case, this information is a formal theory about the relationship between labor-force size and union density. For Stephens, supplementary information comes from historical material and, in particular, the research of Kjellberg (1983). The Bayesian approach allows the information informally introduced into the analysis by Stephens and Wallerstein to enter formally through a prior distribution.

To begin this analysis, we specify two sets of prior information that represent the opinions of the two researchers. Unfortunately, neither theory is sufficiently precise to unambiguously suggest a prior distribution. (We shall return to this issue.) However, it is clear that both Stephens and Wallerstein are confident that left governments assist union growth. We quantify this confidence with a prior mean of .3 and a prior standard deviation of .15 (Table 1). Because the prior 95% confidence interval—.3 ± (1.96 × .15)—does not overlap zero, both researchers are quite confident that left government is unlikely to have a negative effect on unionization. As Wallerstein notes, this effect probably suffers from simultaneity bias because unions increase the electoral success of left-wing parties (1989, 490). Because our substantive focus is on the effects of labor-force size and economic concentration, we treat the left-government variable chiefly as a control and simply note that the coefficient will tend to overestimate the impact of left parties on union density. (The problem of simultaneity can be addressed through the prior distribution, but we reserve this issue for another paper; see, e.g., Leamer 1991.)

Prior opinions of the two researchers differ on the labor-force size and economic concentration effects. Wallerstein has a strong belief in a negative labor-force size effect, although his theory does not suggest how large this effect might be. A few quasi-experimental comparisons using information from outside the sample suggests a plausible prior mean. In 1950, Sweden and Norway shared similar union structures, ethnic homogeneity, and histories of social democracy. The Swedish labor force was about twice as large as the Norwegian, and the unionization gap was about 20 percentage points. If a fifth of the unionization gap (4 percentage points) were attributable to labor-force size, the size effect size would be $4/\ln(2) \approx 5.5$. We get a similar number if we think that about a fifth of postwar American union decline is due to the near-doubling of the U.S. labor force. Choosing a size effect to explain a fifth of the variability in union density reflects, rather arbitrarily, our belief in the quality of the model specification. Four-fifths of the variability in union density is left to the effects of other causes. Confidence in the sign of the effect is reflected in a prior standard deviation that yields a 95% confidence interval that excludes zero.

Stephens believes that economic concentration increases union density, although a prior mean is not obvious from his discussion. Again, we develop a prior based on a quasi-experimental comparison using nonsample information—this time, examining declining economic concentration and union density in the United Kingdom through the 1980s. Measuring economic concentration as the average size of British manufacturing establishments as a ratio of the size of American firms at a fixed point in time, economic concentration in Britain declined by about .3 from the late 1970s to the late 1980s. (See the United Nations Yearbook for Industrial Statistics for 1982 and 1991.) Union density in this period declined by about 15 percentage points. If economic concentration generated about a fifth, or 3 percentage points, of the decline, a plausible prior mean would be $(3/.3) = 10$ percentage points. Confidence in this effect is again supplied through by a prior distribution whose 95% confidence interval does not overlap zero. Note that the style of reasoning behind the specification of these priors is only illustrative. More plausible or more confident specification of prior information would require a much more detailed substantive or theoretical discussion (e.g., Western 1994).

We must also specify a prior for the intercept term, Wallerstein's prior for Stephens's economic concentration variable, and Stephens's prior for Wallerstein's labor-force-size variable. Here, we introduce the idea of *prior ignorance* that characterizes highly uncertain beliefs about parameters. Prior ignorance about the coefficients can be defined by a zero prior mean and a very large prior variance (Leamer 1978, 62). When ignorance is defined in this way for all the coefficients of the model with a so-called diffuse prior, the posterior distribution converges on the conventional least squares result. Thus, the sample data alone will drive the analysis. If the predictors are uncorrelated and ignorance priors are placed on only a subset of the coefficients, the posteriors for those coefficients will also approach the conventional least squares estimates. In short, this ignorance prior does not so much express the belief that a regression coefficient is zero with great uncertainty: rather, the diffuse prior allows the sample information to dominate the prior information in the calculation of the posterior distribution. We place diffuse (or "ignorance") priors on the intercept term, the economic concentration coefficient for Wallerstein's prior, and the labor-force-size coefficient for Stephens' prior.

TABLE 2

Posterior Distributions with Noninformative Prior Information in a Regression Analysis of Union Density

INDEPENDENT VARIABLES	MEAN (S.D.)	5TH PERCENTILE	95TH PERCENTILE
Intercept	97.59 (57.48)	3.04	192.14
Left government	.27 (.08)	.15	.39
Size	−6.46 (3.79)	−12.70	−.22
Concentration	.35 (19.25)	−31.32	32.02

Note: These results are equivalent to the ordinary least squares estimates ($N = 20$)

Results from conventional least squares estimation are in Table 2. Importantly, diagnostics show that the residuals from this least squares fit are approximately normal and the effects, approximately linear (see Fox 1990, 80–87; Hastie and Tibshirani 1990, chaps. 6–7). Thus the data conform to the structure assumed in the normal likelihood function. From a Bayesian perspective these results can be interpreted as the posterior distribution for a noninformative prior distribution. In addition to the mean and standard deviation of the posterior coefficients, we also report normal approximations for the fifth and ninety-fifth percentiles from the posterior distribution (± 1.64 × standard deviation for the coefficient). These percentiles are simply an alternative way of describing the dispersion and location of the posterior distribution. The results suggest a large range of plausible and negative for values for the size effect. The effect of economic concentration is less certain. The posterior mean is close to zero and the posterior distribution stretches over a large range of positive and negative values. In short, the weight of the data favor the effect of size on union density, but neither parameter is estimated with great precision.

The introduction of prior information can sharpen these estimates. Results for Wallerstein's prior are shown in the top panel of Table 3. With the informative prior, the effect of labor-force size is now estimated with greater precision. Given the prior and the data, we can be 90% certain that the coefficient for labor-force size falls between −9 and −2. The improved conditioning of the data is also reflected in the posterior distribution for the economic concentration coefficient. Although Wallerstein's prior for the concentration effect was diffuse, the posterior mean has

TABLE 3

Posterior Distributions with Stephens's and Wallerstein's Informative Priors in a Regression Analysis of Union Density ($N = 20$)

INDEPENDENT VARIABLES	MEAN (S.D.)	5TH PERCENTILE	95TH PERCENTILE
Wallerstein's prior			
Intercept	82.43 (32.83)	28.42	136.43
Left government	.28 (.07)	.17	.39
Logged labor-force size	−5.44 (2.09)	−8.87	−2.00
Economic concentration	4.87 (12.41)	−15.54	25.28
Stephens's prior			
Intercept	70.82 (19.87)	38.13	103.51
Left government	.27 (.07)	.16	.38
Logged labor-force size	−4.79 (1.77)	−7.70	−1.88
Economic concentration	9.38 (4.84)	1.42	17.34

become larger and the standard deviation of the posterior has become about one-third smaller. The confidence region, however, still covers zero.

From Stephens's perspective, when prior information is allocated to the economic concentration coefficient, the posterior mean becomes large and positive and the standard deviation of the posterior distribution shrinks by about four-fifths compared to the least squares estimate. The posterior distribution is now completely in the positive range. Again, as prior information is introduced, inference about both coefficients is generally improved. Although the posterior mean for the size effect has shrunk under Stephens's prior, the confidence region is roughly the same as that obtained with Wallerstein's prior. Both sets of prior beliefs support the inference that labor-force size decreases union density. However, only Stephens's prior supports the conclusion that growing economic concentration increases union density.

Sensitivity Analysis

It might be objected here that the introduction of subjective prior information allows the researchers' prejudices into the analysis, corrupting the results that would be obtained from the sample data alone. This criticism has a long history, demonstrated by Fisher's charge that an experiment interpreted with prior information "would carry with it the serious disadvantage that it would no longer be self-contained, but would depend for its interpretation from experience previously gathered. It could no longer be expected to carry conviction to others lacking this supplementary experience" (1935, 69). A contemporary champion of Fisher's position, Bradley Efron has similarly argued that Bayesian methods "fail to reassure oneself *and others* that the data have been interpreted fairly" (1986, 4; emphasis original). These comments of Fisher and Efron rests on the idea that a "fair data analysis"—a data analysis without prior information—is possible. In practice, however, prior information enters most analyses through coding decisions, transformations, and unreported searches over sets of explanatory variables to obtain results that look sensible in the sense of falling within an expected range of meaningful results. While all data analysts use prior beliefs, Bayesians go some way to making these priors explicit and integrating them systematically into the analysis. In the Bayesian approach then, acknowledged subjectivity is the route to objectivity (deFinetti 1974, 1:5–6).

Still, those objecting to Bayesian inference make a powerful point. Selecting a prior is subjective in the sense that two researchers will not necessarily agree on its specification. For example, at least part of Stephens's prior belief in the effect of economic concentration is based on his historical discussion of the growth of Swedish corporatist bargaining (Stephens and Wallerstein 1991, 944). Other researchers might believe the Swedish experience is unique in the sense of being uninformative about the more general relationship between economic concentration and unionization in comparative perspective. They might argue that Stephens's prior gives undue weight to the lessons of the Swedish case. What is more, priors are specified with a degree of whimsy in applied settings (Leamer 1983). For instance, although Wallerstein's argument led us to a prior labor-force-size effect of mean 5 and standard deviation 2.5, we could readily agree to an alternative prior, say one with the same mean but standard deviation 2.7. In short, neither several researchers nor even the same researcher, in practice, will prefer one prior probability distribution to the exclusion of all others.

Because the choice of prior is subjective in this sense of attracting little consensus, it is important to investigate how the posteriors depend on the priors. If the posteriors are highly sensitive to the priors, this suggests that the sample data add little to the prior information—indeed that inferences are driven by the priors alone. A parallel argument can be made about the relationship between the sample data and the posteriors. If a small number of observations from the sample data are highly influential for the posteriors, the results are similarly unstable, reflecting information about a few cases rather than the whole of the data set in combination with the priors. In sum, because of the joint influence of the prior information and the data on the analysis, a convincing analysis investigates the sensitivity of the posterior to the priors and the data.

The sensitivity of the posterior to the prior could be examined in several ways. One sensitivity analysis,

TABLE 4

Posterior Distributions for Stephens's and Wallerstein's Diffuse Priors in a Regression Analysis of Union Density

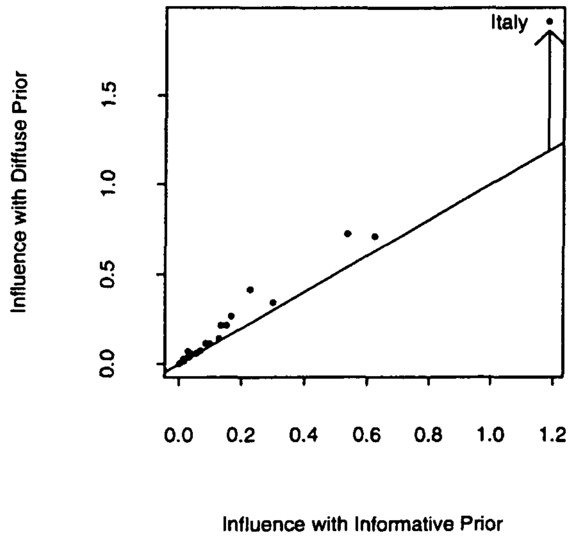
INDEPENDENT VARIABLES	MEAN (S.D.)	5TH PERCENTILE	95TH PERCENTILE
Wallerstein's prior			
Intercept	93.50 (52.02)	7.94	179.07
Left government	.27 (.07)	.15	.39
Logged labor-force size	−6.18 (3.42)	−11.81	−.56
Economic concentration	1.60 (17.68)	−27.49	30.68
Stephens's prior			
Intercept	80.88 (38.14)	18.15	143.62
Left government	.27 (.07)	.15	.39
Logged labor-force size	−5.42 (2.69)	−9.85	−.99
Economic concentration	6.11 (12.22)	−13.99	26.20

Note: *The diffuse priors are the informative priors multiplied by 10 (N =* 20).

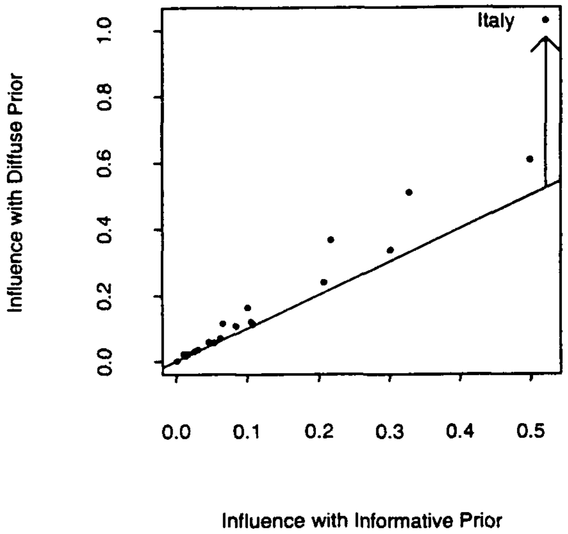
FIGURE 1

Sensitivity of Bayesian Regression Results to Prior Information and the Sample Data.

(a) Wallerstein's prior



(b) Stephens' prior



Note: The plot shows Bayesian influence statistics for informative priors (horizontal axis) and diffuse priors (vertical axis). If the sample data from a country have the same influence on the final results, regardless of prior, it will fall on the solid 45-degree line. Italy is an outlier, becoming highly influential as prior information is removed from the analysis.

sometimes called "extreme bounds analysis," investigates variability in the posterior as prior variances are allowed to range from zero to infinity while the prior means are fixed at zero (Leamer 1983). In our application, with nonzero prior means, we simply describe the sensitivity of the posterior to a reduction in the prior information through an increase in the prior variances. An alternative approach might manipulate the prior means, although this should be pursued in the spirit of a sensitivity analysis, leaving a priori uncertainty about the regression coefficients to be expressed in their prior variances.

We report two more sets of posterior distributions for a diffuse prior with the prior variances multiplied by 10. Under the diffuse version of Wallerstein's prior, Table 4 shows that the labor-force-size coefficient becomes larger but substantially more imprecise. The 90% confidence region still excludes—but now borders on—zero. The posterior for economic concentration is now effectively centered over zero. For Stephens's diffuse prior, the posterior mean of the concentration coefficient shrinks by about a third, and the standard deviation of the coefficient increases by about three times compared to Stephens's informative prior. The 90% confidence interval now includes a large range of negative values expressing great uncertainty about the effect of economic concentration on union density. Finally, similar to Wallerstein's diffuse prior, the posterior for size gets slightly larger but substantially more imprecise compared to the posteriors based on the informative priors. To summarize, inferences under both priors become considerably more uncertain as nonsample information is removed from the analysis. A strong inference about the sign of the economic concentration effect depends crucially on the informative version of Stephens's prior.

The sensitivity of the posteriors to the data can be

TABLE 5

Posterior Distributions with Informative and Diffuse Priors in a Regression Analysis of Union Density

INDEPENDENT VARIABLES	INFORMATIVE PRIOR			DIFFUSE PRIOR		
	MEAN (S.D.)	5TH PERCENTILE	95TH PERCENTILE	MEAN (S.D.)	5TH PERCENTILE	95TH PERCENTILE
Wallerstein's prior						
Intercept	54.16 (34.61)	−2.76	111.09	31.19 (56.50)	−61.75	124.12
Left government	.29 (.06)	.19	.40	.29 (.07)	.18	.41
Logged labor-force size	−4.19 (2.12)	−7.67	−.70	−2.71 (3.57)	−8.58	3.16
Economic concentration	16.29 (13.02)	−5.12	37.70	23.72 (19.41)	−8.20	55.64
Stephens's prior						
Intercept	67.53 (19.01)	36.27	98.80	53.15 (38.93)	−10.88	117.18
Left government	.30 (.06)	.19	.40	.30 (.07)	.18	.41
Logged labor-force size	−4.88 (1.64)	−7.58	−2.17	−4.01 (2.64)	−8.34	.33
Economic concentration	10.87 (4.87)	2.86	18.87	15.88 (12.72)	−5.04	36.80

Note: In this analysis, the outlying case of Italy is omitted. The diffuse priors are the informative priors multiplied by 10 (N = 19). Least squares estimation with Italy excluded yields the following result (standard errors in parentheses): Union Density = 22.08 (63.11) + .30 (.07) × Left Government − 2.12 (4.00) × Size + 26.66 (21.41) × Concentration.

investigated with Bayesian regression diagnostics. Pettit and Smith (1986) discuss a statistic that indicates the influence of each sample observation (or set of observations) on the joint distribution of the posterior (see Appendix). This diagnostic indicates that Italy is highly influential under both informative priors. Both dimensions of the sensitivity analysis—sensitivity to the prior and sensitivity to the data—are shown in Figure 1. Here, influence statistics from the informative prior are plotted against influence statistics from the diffuse prior. If the sample data from a country have the same influence on the final results, regardless of the level of prior information, the country will fall on the solid 45-degree line. However, as the prior information is reduced, the sample data generally have an increasing impact on the posterior, so that the quantitative information from most countries becomes more influential as the prior information recedes. Italy grows disproportionately in its influence on the posterior. When prior information is eliminated, this country is shifted upward on the plot and indeed drives a large part of the story.

Table 5 summarizes the posterior distribution of the coefficients when Italy is omitted. These results show that Italy is influential for the posterior means of the size and concentration coefficients but has little impact on the posterior variances. Thus our posterior uncertainty changes little as a result of deleting Italy, but the range of values over which we are uncertain changes considerably. The new posterior distributions for the reduced data set indicate that Italy was driving up the posterior mean for the size effect but suppressing the posterior mean for concentration. Under Wallerstein's prior, the bulk of the posterior for the size coefficient remains negative, although the magnitude of the posterior mean declines by about a fifth. By contrast, the posterior mean for economic concentration increases by four times when Italy is omitted. Still, uncertainty about the effect remains substantial and the 90% confidence interval still includes zero.

Interestingly, Stephens's prior produces stronger evidence for the effect of size than Wallerstein's prior when Italy is omitted. This can be explained by noting that when there is strong dependency between two variables (size and concentration in this case), prior information for one can result in a sharper estimate for the effects of the other. In effect, an informative prior for the concentration coefficient frees up independent information for the size coefficient. (For a discussion and further examples, see Belsley 1991, 311, 318). This finding illustrates an implication of our discussion of collinearity: when variables are correlated, inference about one coefficient depends on prior information about others (Leamer 1978, 176). Like the size effect under Stephens's prior, the concentration effect remains fairly robust to the exclusion of Italy. Sensitivity analysis on the reduced data set also resembles the full sample analysis, indicating the crucial importance of prior information for inference about the signs of the size and concentration effects. With diffuse priors in the reduced sample, all posterior size and concentration effects overlap zero.

CONCLUSION

We have tried to do two things. First, we have tried to identify two problems with conventional statistical inference in comparative research: weak and nonstochastic data. Second, we have outlined a Bayesian approach to statistical inference that offers unified and principled solutions to these disparate problems. Regardless of the merits of the Bayesian approach to statistical inference, the problems with conventional frequentist inference in comparative research that we identify will not go away. Conventional inference, like Bayesian inference, is based on a positive theory of how data are generated. This is not commonly recognized. In practice, data analysis also involves the use of extensive prior information. Again, this is not widely acknowledged. A chief concern of ours, then, is to problematize conventional statistical practice in an area for which conventional statistical inference was not particularly designed.

The Bayesian alternative that we propose is one way of addressing the problems of statistical inference in comparative research. It is based on a subjective probability concept that does not rely on data generated by a repeatable mechanism. The theory of data generation behind Bayesian inference is thus more general than the theory of data generation in conventional inference. Sources of uncertainty in the Bayesian approach are not restricted to—but could include—repetitious social processes. The other advantage of the Bayesian approach in the context of comparative research is the formal incorporation of prior information. When sample data are weak, prior information provides a useful supplement to the analysis. Still, the subjective choice of prior is an important weakness of Bayesian practice. The consequences of this weakness can be limited by surveying the sensitivity of conclusions to a broad range of prior beliefs and to subsets of the sample.

To summarize the data analysis, inferences about the negative effect of labor-force size and the positive effect of economic concentration on unionization are sensitive to the priors and the data. Analysis of the full sample showed that evidence for the size effect is consistent with a large set of informative and diffuse priors. Stephens's argument for a positive concentration coefficient, on the other hand, depends decisively on a prior belief in this effect. Italy drives confidence in the size effect and contributes uncertainty to the impact of economic concentration. When Italy is excluded from the analysis, all the results are weakened substantially, and no sign inference about either the concentration or size effect is sustained under the diffuse priors.

On balance, the sample data alone are not sufficiently informative to decide whether Wallerstein or Stephens is right. A resolution requires more information, which can be supplied through a prior distribution. The plausibility of the results is thus closely linked to the plausibility of the priors. Although this conclusion may appear unsatisfying, we would argue that it demands a lot from 20 data points to resolve a question of broad comparative scope concerning a complex process of institution building. More optimistically, if we allow information like Stephens's qualitative historical account of the impact of economic concentration on early Swedish unionization, a stronger set of results can be obtained. Thus, the data analysis underlines the importance of case studies and comparative histories to assist in the interpretation of quantitative evidence. While this is not news for comparative researchers, the Bayesian approach that we describe permits the transparent incorporation of this supplementary and influential information.

APPENDIX: BAYESIAN REGRESSION DIAGNOSTICS

Following the notation in the text, where s^2 is the sample estimate of the residual variance, Pettit and Smith's (1986) Bayesian influence statistic, I_i, expressing the influence of the ith observation on the joint posterior distribution of the regression coefficients, is

$$I_i = \frac{u_i}{2(1-u_i)}\left[u_i + \frac{e_i^2(2-u_i)}{s^2(1-u_i)}\right],$$

where $u = \text{diag}[s^{-2}(XV^*X')]$, and e is a vector of posterior residuals (the difference between the observed response and the fitted values from the observed data and posterior regression coefficients).

The I-statistic has parallels with conventional diagnostics. The matrix from which u is extracted can be understood as a "posterior hat-matrix" with the leverages of the sample observations on the posterior parameters along the main diagonal (Pettit 1985, 189; for discussion of leverage and the hat-matrix, see Cook and Weisberg 1982, 11–22). The scaled squared residuals in the expression for I can be thought of as squared "posterior studentized residuals" (Pettit and Smith 1986, 485).

S-Plus and GAUSS routines to calculate posterior regression parameters, confidence intervals, and Pettit and Smith's influence statistic are available on request. Press 1989 contains a useful software review.

Note

We thank David Gow for his comments on an earlier draft, and Richard Berk and Larry Bartels for their (seemingly unrelated) influences on our thinking about statistical inference. Chris Achen, Neal Beck, David Epstein, Don Green, Gary King, Sharyn O'Hallorhan, Marco Steenbergen and seminar participants at the University of California, Los Angeles, Department of Sociology provided helpful comments and criticisms. Earlier versions of this paper were presented at the annual

meetings of the Midwest Political Science Association, Chicago, 1993 and the Tenth Annual Political Methodology Conference, Tallahassee, 1993.

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CONTROVERSY

American Political Science Review Vol. 88, No. 2 June 1994

PUBLIC SPHERE, POSTMODERNISM AND POLEMIC

Theories of the public sphere, as standardly formulated, aim to specify the minimal, necessary conditions for a discursive realm free of coercion or manipulation. In his article in this Review in September 1992, Dana Villa urged us to reconsider this standard account. He argued that when read in light of postmodernist theory, Hannah Arendt provides the basis for a revised conception of the public sphere that privileges plurality and difference over consensus. Jim Johnson suggests that Villa's analysis is a thinly veiled polemic against critical theory. Johnson argues that, as critique, Villa's argument is neither decisive nor encompassing, and that as polemic it blinds Villa to potentially fruitful disagreements with critical theorists. Villa replies that Johnson misses the synthetic thrust of the original article because he identified public realm theory too narrowly with Habermas. Thus, he misconstrues the dialogue Villa sought to facilitate between Arendt and postmodernism.

COMMENT

> There are the sterilizing effects: Has anyone ever seen a new idea come out of a polemic?—Michel Foucault, *Polemics, Politics, and Problemizations*

In his article "Postmodernism and the Public Sphere," Dana Villa (1992a) disputes the way political theorists currently conceptualize the public sphere. In particular, he objects to those who identify Hannah Arendt's rendering of the public sphere as a model for "a dialogical, consensus based politics" (see Villa 1992b, 274). Jürgen Habermas is, if not the only, certainly the main culprit here.[1] His "polemical stance," according to Villa, both exaggerates the opposition between critical theory and postmodernism and fosters common misperceptions of Arendt's relation to these competing theoretical projects (p. 713). Villa wants to rectify these misconceptions. He aims to answer Habermas' polemic and thereby to prompt us to reconsider the relation between postmodernism and Arendt's defense of publicity (pp. 713, 717). His strategy has three parts. First, Villa advances a set of postmodern objections intended to discredit Habermas' normative ideal of the public sphere. Second, he seeks to show how Arendt's defense of the public sphere—interpreted as the locus of "a politics that stresses plurality, difference, spontaneity, and initiation against the regularizing apparatus of consensus" (p. 719)—more successfully meets these objections. Third, on that basis, he seeks to establish, albeit with certain important reservations, a set of affinities between Arendt's work and postmodern theory.[2]

In short, Villa aims to deploy postmodern skepticism against the rationalist modernism embraced by Habermas. Then, having discredited critical theory, he seeks to distance himself from what he takes to be the unattractive positions of his erstwhile postmodern allies. In the course of his argument, however, Villa himself resorts to polemic. The most obvious cost of his polemical stance is that it blinds him to possibilities for an enlarged and enhanced debate, one that might lead beyond the present impasse between postmodernism and critical theory. I try to convey the extent of this cost by indicating, without fully elaborating, several points of engagement that Villa overlooks. I present these as *questions*; the problem is not that Villa fails to answer these questions but that his polemic prevents him from raising them (or similar ones) in the first place.

The issue here is not whether one or another side gets Arendt "right." Villa himself concedes that Habermas captures a central theme of Arendt's thought (p. 713). And her work is clearly rich enough to sustain more than one defensible reading. So my purpose is not to challenge Villa's interpretation of Arendt. Similarly, the issue is not whether Habermas' portrait of the public sphere (or the larger theoretical project of which it is a part) is above criticism. It surely is not. And my intention is not uncritically to endorse his project. The issue, rather, can be captured in three related propositions: (1) the postmodern arguments that Villa advances against critical theory are not as decisive as he seems to think; consequently, (2) the range of theoretical alternatives that emerge from Villa's proposed rethinking of Arendt's relation to postmodern theory is unjustifiably truncated; and (3) (perhaps most importantly) polemic is not a fruitful mode for conducting theoretical disagreements. I shall elaborate on each of these propositions in turn.

RESPONSES TO POSTMODERN OBJECTIONS

Villa advances what he considers to be "the three most basic and forceful" postmodern objections to the normative ideal of the public sphere as a site of unconstrained political debate and deliberation (p. 712). The *power objection* challenges the ideal of a public sphere as the locus of a dialogical politics. He disparages this ideal for "its naive reliance upon conditions of symmetry, nonhierarchy, and reciprocity as adequate guarantees of a 'coercion-free' space" (p. 715). He contends that it neglects the operation of what Foucault calls disciplinary power, which, because it functions through insidious, continuous, ubiquitous mechanisms of surveillance and normalization, demonstrates that "hierarchy and asymmetry (the two primary villains according to public realm theory) are not essential" to the operation of power in modern society (p. 715). Consequently, the normative ideal of a public sphere is a radically inappropriate remedy for disciplinary power.

The *epistemological objection* contends that the ideal of a public sphere is "parasitic on an Enlightenment metanarrative of emancipation" (p. 716). As such it is at odds with the defining feature of the "postmodern age," namely, a widespread "incredulity toward metanarratives" generally and toward that of political emancipation in particular (Lyotard 1984, 3, xxiv). From this perspective, Habermas' "attempt to delineate the public sphere in terms of a specific form of interaction and the rationality appropriate to it" appears especially suspect. This is because it threatens to regularize—and thereby suppress—not only "the irreducible heterogeneity of language games" but the plurality of voices that might participate in such games (p. 716).

Villa thinks that the third, *ontological objection*, is "in some ways the most telling" criticism that postmodern theorists level at the ideal of a public sphere (p. 712). It questions "the specific reality claimed for the public sphere and the objects within it" (pp. 712, 716). And, while Villa does not develop this theme, he intimates that it highlights the dubious (because abstract and procedural) characterization that Habermas gives to the public sphere.

I will now suggest how critical theorists might respond to these postmodern objections. In each case, they confront pressing and difficult issues. But in no instance are the objections Villa raises to critical theory as decisive as he would have us believe. Thus they provide no clear warrant for preferring Arendt's rendering of the public sphere (as Villa reinterprets it) to the normative ideal that critical theorists advance. I do not present these responses as decisive refutations. I simply wish to indicate that despite Villa's conclusive tone, room exists for potentially productive disagreement on the issues that he raises. Again, my aim is to highlight missed opportunities—to raise questions, not to provide definitive answers.

Power

Habermas (1989a) locates the rise of the bourgeois public sphere in the eighteenth century. Foucault locates the perfection of disciplinary power during the same historical period. He argues that "the development and generalization of disciplinary mechanisms constituted the other, dark side" of the processes that Habermas traces. More strongly, Foucault asserts that "those systems of micro-power that are *essentially nonegalitarian and asymmetrical* that we call the disciplines" provided the necessary, unavoidable "foundation" for the emergence of a liberal democratic politics. And he goes on to explain that disciplinary institutions *"have the precise role of introducing insuperable asymmetries and excluding reciprocities"* (Foucault 1979, 222 emphasis mine).[3] His most telling example of the asymmetrical, hierarchical, nonreciprocal nature of disciplinary power is Jeremy Bentham's panopticon. In this quintessential disciplinary institution, "each individual, in his place, is securely confined to a cell from which he is seen from the front by a supervisor; but the side walls prevent him from coming into contact with his companions. *He is seen but he does not see; he is the object of information, never a subject of communication*" (p. 200 emphasis mine).

These passages are neither obscure nor easy to reconcile with the conventional postmodern interpretation of Foucault that Villa accepts without hesitation. They also are difficult to reconcile with the categorical assertion Villa makes on the basis of that interpretation, namely, that hierarchy and asymmetry are not essential to the functioning of disciplinary power (p. 715). The issue here, once again, is not simply who gets Foucault "right." The issue, rather, is whether Villa's objection to the normative ideal of a public sphere is well specified. If, as the quoted passages suggest, hierarchical, asymmetric, nonreciprocal relations are central to even disciplinary forms of power, the project of specifying an idealized model of unconstrained political discourse, though difficult in ways that I shall indicate, is hardly as ill advised as Villa makes out.[4]

A less conventional and, I would argue, less polemical interpretation of Foucault opens possibilities for debate. For instance, if the perfection of disciplinary institutions represents the "dark side" of political modernity, do such institutions, as Foucault seems to suggest, find their inescapable normative counterpoint in claims to reciprocal, symmetric and equal communication?[5] Conversely, how, if at all, could critical theorists articulate the possibility that uncoerced public communication might, as Habermas intimates (1992, 74–75), be extended from overseeing familiar political and economic institutions (e.g., state and market) to interrogating other decentralized, but nonetheless systematic operations of social power (e.g., social norms)?

Epistemology

As Villa reports, Lyotard "famously *defines*" postmodernity as consisting in a thoroughgoing suspicion toward modernist metanarratives generally and toward the metanarrative of political emancipation in particular (p. 716, emphasis mine). But this definition of the postmodern age seemingly neglects a quite plausible yet disconfirming interpretation of the most momentous political events of our time.[6] As Habermas points out, the revolutions of 1989 in East Central Europe were hardly made under the banner of postmodern skepticism (1990a, 7–8). They drew upon the well known repertoire of modern political protest. And they were animated—if not exclusively, at least conspicuously—by key ingredients in the modern narrative, namely the legitimating ideas of truth, justice, liberty, and popular sovereignty.[7] Any account of so complex a set of events is necessarily incomplete, hence contestable.[8] Postmodern theorists will no doubt advance rival interpretations. But is it not at least arguable that, as Habermas suggests, modernity is "extending its borders" rather than retreating in the face of postmodern skepticism?[9]

Empirical evidence, however desperately needed in disagreements of this sort, is unlikely to resolve this question. But it surely provides warrant for questioning whether Lyotard's *definition* of the postmodern condition by itself embarrasses efforts to articulate a normative ideal of a public sphere. As events suggest, postmodernists seem greatly to exaggerate the demise of what they call the modern metanarrative of emancipation. What, then, prevents critical theorists from drawing upon that narrative, or its components, to ground criticisms of the homogenizing, exclusionary potential that postmodern theorists hold is inherent in any vision of a democratic public sphere?[10]

Ontology

Villa himself concedes that this third objection is less relevant to Habermas than to Arendt (p. 716). But he complains that "Habermas is able to evade it" only by grounding his defense of publicity in "an abstractly procedural account of discursive rationality" (p. 713). Villa resists this "evasion" without explicitly stating why. Yet he nowhere asks whether critical theorists are necessarily committed to Habermas' quite strong formulation of communicative reason (McCarthy 1992, 3, 181–99).

Critical theorists surely aspire to ground their ideal of the public sphere in a notion of communicative reason. But do they need to understand this as a *regulative* idea, something toward which practices in fact aim? Might they not understand it, more modestly, as a *critical* ideal—a counterfactual vantage point from which to assess and challenge extant political practices and institutions? Foucault for example, countenances the latter possibility. He concedes that "the idea of a consensual politics may indeed at a given moment serve either as a regulatory principle or better yet as a critical principle with respect to other political forms," subsequently settling on the second interpretation. Thus the ideal of a consensus-based politics is a critical idea to maintain at all times: first, one asks oneself "what proportion of nonconsensuality is implied in such a power relation, and whether that degree of nonconsensuality is necessary or not, and then one may question every power relation to that extent" (1984, 378–79).[11]

This defense of the public sphere raises pressing theoretical questions. Can critical theorists articulate the seemingly common normative criteria that both tacitly inform Foucault's critique of disciplinary power *and* explicitly undergird their own defense of the public sphere?[12] Can they specify more persuasively the individual competences and social conditions necessary to make understanding, if still improbable, at least *conceivable* (Johnson 1991, 1993)? Are there reasons to think that critical theorists cannot address such questions? Villa surely does not offer any.

In the end, then, Villa's objections to critical theory are contestable in ways I have indicated. Given his confident tone, this conclusion is disturbing in itself. But the sorts of question I have raised highlight a further, especially baleful consequence of his argument. Villa unjustifiably circumscribes the range of viable theoretical alternatives precisely because he never even entertains the possibility that critical theorists might advance a more supple, modernist defense of the public sphere.

NIETZSCHE *OR* ARISTOTLE?

MacIntyre (1981) posed this dilemma more than a decade ago. Villa concludes that we face an updated but similarly glaring antimodernist predicament: postmodern skepticism *or* premodern nostalgia (p. 719).[13] Simply because he arrives via an alternate route neither makes this choice appealing nor renders it inevitable. Indeed, it is ironic to find Villa calling on postmodern theory in order to recapitulate existing dichotomies. This, however, is what his proposed rethinking of the relation between postmodernism and the public sphere leads him to do.

There is one condition under which the impoverished range of theoretical alternatives that Villa offers is justifiable. Neither critical theorists nor others must be able to advance a cogent modernist defense of the public sphere, where one minimal criterion of cogency is that any such defense address postmodernist concerns. Does this condition hold? Villa never asks. Instead, he actively suppresses opportunities to actually engage critical theory and thereby to enhance rather than stifle theoretical discussion.[14] I have already implicitly identified several such missed chances. Here I will very briefly suggest two more.

First, Villa might push critical theorists to develop the *remarkable* rather than the predictable themes in Habermas' essay on Arendt.[15] I refer to Habermas' firm reminder that "strategic action has taken place also within the city walls" and that therefore any adequate conception of politics must incorporate a robust, nonresidual role for strategic interaction (Habermas 1983, 180, 183). Might critical theorists take this observation as an opening from which to explore how (or whether) to address what postmodern theorists call the "agonistic" dimensions of social and political interaction?[16]

Second, Villa might push critical theorists to confront an apparent, especially relevant tension, internal to Habermas' theory of communicative action. On the one hand, Habermas presumes that actors are communicatively competent—that they not only can, but necessarily do, embed their utterances in a system of contestable validity claims. On the other hand, he also insists that actors are endowed with "linguistic competence" and that this mastery of the grammatical rules of a natural language grounds a capacity not simply to formulate "comprehensible" utterances but to do so in innovative ways and under novel circumstances (Habermas 1979, 20, 26–27). Does this tension between constraint and creativity provide an opening from which critical theorists could more adequately address postmodern qualms about the potential homogenizing effects of communicative reason?

These are hard questions. Critical theorists need to pursue answers to this sort of question in order plausibly to defend the normative ideal of the public sphere and thereby avoid Villa's stark antimodernist predicament. Villa offers no reason to think that they cannot do so. Indeed, because he is engaged in a polemic Villa never even acknowledges the opening these sorts of question provide. Consequently, despite his professed concern for the exaggerated sense of opposition between postmodernism and critical theory, Villa's argument has the effect of exacerbating rather than easing existing theoretical divisions.

POLEMIC: HOW NOT TO PURSUE POLITICAL THEORY

Villa complains that Habermas adopts a "polemical stance" toward postmodern theorists. His complaint seems warranted. Habermas, who is well known for openly engaging theoretical rivals and critics, is perhaps uncharacteristically harsh when he discusses postmodern theory.[17] Villa's complaint also is ironic, for it tacitly invokes an ideal of free and open debate that Villa subverts in his own practice.

Consider in this respect the "essential" distinction Foucault (1984, 381–82) draws between entering discussion and engaging in polemic. Discussion is "a game that is at once pleasant and difficult." In it participants implicitly undertake commitments entailed by "the acceptance of dialogue" and avail themselves only of rights that "are in some sense immanent" in "the dialogue situation" itself. In polemic, by contrast, the aim is not simply "to wage war" but to regard "that struggle as just undertaking." The polemicist proceeds unconstrained by mutual rights and commitments, and treats each interlocutor not as "a partner in the search for truth, but an adversary, an enemy who is wrong."

Villa is waging a just war in Foucault's sense. To that end he deploys an arsenal of polemical weapons. He offers as conclusive readings of texts that, however conventional they may be, can at best be held tentatively. He argues by definition. He presents as inevitable a theoretical predicament that in actuality is an artifact of his own argument. And even if we grant that Habermas adopts a polemical stance toward postmodern theory, Villa never acknowledges—let alone addresses—other critical theorists who adopt less contentious approaches.[18] By proceeding in this way Villa encounters performative contradiction: he endorses postmodern impertinence, parody, and skepticism against a suppressed normative background that alone gives credence to his complaints about polemic. But more importantly, the rigors of the campaign blind Villa to the possibility of the sort of principled and productive discussion that Foucault professed to enjoy.[19] His polemical stance prevents him from asking—let alone resolving—the sorts of question I have raised in this comment. With the possible exception of Arendt, Villa leaves all parties in their original places. The primary difference is that their positions are now entrenched, rigidified, less susceptible to reformulation. By his example, then, Villa also regrettably answers *no* to the question Foucault poses in my epigram. Critical theorists, postmodernists, and others should take note.

James Johnson

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RESPONSE

Toward the end of his response to my essay on public realm theory and postmodernism, James Johnson complains that "with the possible exception of Arendt, Villa leaves all parties in their original places." This is, for reasons I shall detail below, a revealing and remarkable claim, one that implicitly dismisses the public realm theory of Hannah Arendt and endows Habermas' theory with an unquestionable hegemonic status. Johnson finds no "new ideas" in my essay because he identifies public realm theory solely with Habermasian theory. Hence, he misses the synthetic thrust of my essay, which attempts to overcome the hardened opposition between public realm theory and postmodernism specifically by opting for Arendt's theorization of an agonistic public sphere over Habermasian proceduralism.

Let us look, first, at the charges. Johnson accuses me of eschewing productive discussion in favor of polemic, a choice that dooms any attempt to rethink positively the map of contemporary theory. According to Johnson, my polemical intent guarantees that no genuinely new configuration between modern public realm theorists and their postmodern opponents will appear; rather, old battle lines will simply be redrawn, old oppositions sharpened, and the potential for fruitful dialogue dismissed. Johnson asserts that the reader of my essay who expects an *aufhebung* of the modern–postmodern debate (or, at the very least, a step forward) will be sorely disappointed. Instead of overcoming the baneful *either/ors* of contemporary theory, Johnson thinks my essay merely shifts them, concluding with a dichotomy

(Nietzsche or Aristotle) every bit as exhaustive and undialectical as the Habermasian one I attack.

What is interesting about this charge is the way it surreptiously transfers Habermas-size ambitions to me, such as the desire to provide an exhaustive mapping of the world of contemporary theory. This desire clearly animates *The Philosophical Discourse of Modernity*, with the binary of "subject-centered reason" and its critics on the one hand and the "paradigm of mutual understanding" on the other. Johnson agrees with me that the thick black line that Habermas draws between the two paradigms is reductionist but thinks that I am trying to refute Habermas simply by drawing another, equally reductionist, boundary. Johnson sees such boundary drawing as perpetuating a theoretical Punch and Judy show. He wants us to put down our paint brushes and get on with the serious business of working out the kinks of critical theory in general and public realm theory in particular.

My essay does not substitute *my* global map of theory of Habermas'; rather, it questions the adequacy of Habermas' mapping and suggests that the rigidity of his dichotomy has underscored a largely *false* opposition between public realm theory and postmodernism/poststucturalism. As I point out, it is to the discredit of many postmodern theorists that they largely accept this opposition as fact. My essay suggests that this opposition has been reified; that the politics of public realm theory are more complex, ambiguous, and subtle than many Habermasians or postmoderns assume; and that the unique public realm theory of Hannah Arendt opens channels of communication between public realm theory and postmodernism in a way foreclosed by Habermas.

My goal in presenting an overview of public realm theory as construed by the Habermasians and various postmodern objections to such theory was not to *resolve* this difference but to show the way the terms of the debate had been set. The strategy of the essay is first to focus on the opposition and then to show how Arendt's public realm theory straddles it. It is because Arendt theorizes an *agonistic* public sphere that many of the more typical postmodern objections to public realm theory have little purchase on her. Thus the heart of my essay is found in its distinction between public realm theory as practiced by Habermas and public realm theory as practiced by Arendt (a distinction that has been largely obscured by the Habermasian appropriation of Arendt). With *this* distinction in mind, new possibilities and configurations become possible between postmodernism and (a more Arendtian conception of) public realm theory—hence my emphasis on how *Arendt's* theory can answer many of the objections raised by postmodernism/poststructuralism to public realm theory of the Habermasian sort. Of course, there are differences between Arendt and Foucault, Lyotard and others—differences I tried to highlight with my final contrast between Arendt's nostalgia for a public reality and the postmodern affirmation of the blurring of boundaries (a decidedly *un*-Arendtian ambition).

My essay, then, does not offer up the choice between "postmodern skepticism" and "nostalgia for the premodern" as a substitute for the Habermasian dichotomy between public realm theory and the concerns of the postmoderns. Rather, it suggests an unexplored avenue of dialogue or synthesis, while reminding the reader that Arendt's agonism retains its specificity. In other words, thinking the public realm with Arendt rather than Habermas enables us to overcome many of the roadblocks to the kind of theoretical dialogue or synthesis Johnson thinks so salutary. Why, then, the fuss about polemic and the *shutting down* of theoretical dialogue? Why does Johnson read my essay as *repudiating*, rather than furthering, a potential dialogue between postmodernism and public realm theory?

The obvious answer is that Johnson identifies "public realm theory" with the Habermasian project, broadly construed. If one begins with the assumption that Habermas has a monopoly on public realm theory, that he has established a normative paradigm that englobes public realm theory tout court, then it becomes possible to say, with Johnson, that "with the possible exception of Arendt, Villa leaves all parties in their original places." Unfortunately, nothing of the sort takes place. I have, on the contrary, shown that the "abyss" that separates postmodernism and public realm theory is much narrower than supposed. What irks Johnson (and leads him to miss the synthetic character of my essay) is that I do not share his presumption that public realm theory *is* Habermasian critical theory or that Habermas establishes a theoretical horizon within which *all* talk of the public sphere must invariably occur. Following Arendt, I think there are other, more fruitful ways of talking about the public sphere—ways that keep plurality, debate, and difference alive rather than seeking to shut them down via a formalistic decision procedure. Because Johnson believes that the only *relevant* dialogue is between Habermas and his critics, he dismisses the dialogue I am attempting to facilitate between Arendt and postmodernism. Needless to say, I think this stacks the dialogical deck a little too much: there are other, equally important conversations to be had in contemporary theory.

Once we acknowledge that the modern–postmodern debate opens up a number of conversational avenues worth exploring, not just one, Johnson's lengthy list of complaints is put into perspective. Those who, like myself, find postmodern objections to public realm theory à la Habermas persuasive are apt to seek out different, less global, models of the public sphere. Those who, like Johnson, find these objections either unconvincing or answerable may well remain committed to Habermas' discursive model. The conversation one pursues largely depends upon one's reaction to the various criticisms voiced by Foucault, Lyotard, and others. My essay assumes that these criticisms have some bite and that public realm theory would do well to take account of them, specifically by abandoning a strong normative conception of the public sphere for a more flexible, phenomenological approach. Johnson, given his allegiance to Habermas' discursive paradigm, proposes a different sort of "taking account"—one that continues to treat the discursive model of the public realm as normative. Here we can agree to disagree. What I find peculiar is Johnson's insistence that I write the essay *he* would have written on the topic of postmodernism and the public sphere. Having failed in this regard, my essay is branded mere polemic.

Johnson accuses me of "waging war" rather than engaging in "the search for truth" because the conversation I wish to promote is not the one he deems important. His deployment of Foucault's comment about polemic is designed as a kind of coup de grace—a damning at the hands of the "master" that places my essay outside the bounds of civilized theoretical discourse. Even Foucault, Johnson triumphantly declares, believed in the Socratic virtues. It may come as a surprise to Johnson, but so do I. However, like Foucault—and, indeed, like Arendt—I am all too aware of how the Socratic virtues can be codified as an exclusionary and silencing discursive rationality. Johnson's "nonpolemical" comments end by banishing me from the dialogue of political theory. The irony, I presume, is intended.

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Notes

Thanks to Jack Knight and Stephen White for very helpful comments on earlier drafts.

1. Habermas is not Villa's only culprit. Other critical theorists, such as Richard Bernstein and Seyla Benhabib, are also at fault (p. 714). And many postmodernists are complicit insofar as they accept Habermas' interpretation (p. 719, n. 6).

2. Specifically, Villa claims that Arendt "avoids the relativist consequences of postmodern pluralism" by appealing to a shared understanding of the world and that she adopts a different "attitude" than do postmodernists to the subversion of "a distinctly *political* realm" in the modern world (p. 719). These reservations about Arendt's relation to postmodern theory come out more clearly elsewhere (see Villa 1992b).

3. Thus, Foucault explains that as it emerged, disciplinary power had to "solve a number of problems" that other forms of power were ill equipped to address: specifically, it had to "master all the forces that are formed from the very constitution of an organized multiplicity; it must neutralize the effects of counter-power that spring from them and which form a resistance to the power that wishes to dominate it: agitations, revolts, spontaneous organizations, coalitions—*anything that may establish horizontal conjunctions*" (Foucault 1979, 219, emphasis mine). And since, as Foucault insists, "there are no relations of power without resistances" (1980, 142), disciplinary mechanisms must continuously perform this task of introducing asymmetries and subverting reciprocities.

4. This is especially so because, as Foucault explicitly claims, disciplinary mechanisms do not exhaust the entire range of modern power relations (1980, 106; 1984, 380).

5. Jacques Derrida once explained to an interviewer, "I cannot conceive of a radical critique which would not be ultimately motivated by some sort of affirmation, acknowledged or not" (Kearney 1984, 118). Foucault, despite conventional readings of his work, implicitly seems to exemplify this reminder.

6. This statement might seem miscast to those who see Lyotard as making a normative rather than an empirical argument. But this objection is difficult to sustain. While Lyotard clearly advances a normative argument, that argument just as clearly presupposes empirical—and hence fallible—claims regarding the nature of the "postmodern world" (1984, 41). Consider, for example, his claim that "the word postmodern . . . designates the state of our culture" and his explicitly offering his views on the altered status of knowledge in our postmodern culture as a "working hypothesis" (Lyotard, 1984, xxiii, 3). Or consider that he himself invokes a litany of Western and Eastern European events as "evidence" that the modern narrative of emancipation has not simply "been forsaken or forgotten, but destroyed, 'liquidated' " (Lyotard 1992, 28–29, 18).

7. Similarly, Western responses to 1989 and its aftermath, where they have not been purely opportunistic, have arguably been animated by modernist impulses.

8. Postmodern theorists will no doubt distrust Habermas' reading of events. Before too quickly dismissing his account, however, they might consider Timothy Garton Ash's firsthand assessment of how distinctly modernist ideas influenced Central European events in 1989 (1990, 105, 154).

9. In Central Europe today, if modernity is besieged, it is by the sort of ethnic hostility, xenophobia, and so on that transforms the "difference" and "otherness" that postmodern theorists celebrate into violent enmity (Tismanaenu 1992, 3, 279–88). Nevertheless, besieged does not mean overwhelmed: Do either postmodern theorists or Arendt have a superior, coherent response to this trend that does not derive its force from the modern narrative?

10. I have in mind here the task of elaborating on the possibility of what, in his recent political writings and interviews, Habermas repeatedly refers to as "autonomous public *spheres*" (Habermas 1989 b; 67; 1990a, 18; 1992, 183; emphasis mine). Nancy Fraser (1992) has initiated this task in a fair but pointed criticism of Habermas. Villa views her efforts as "quasi-Lyotardian" (p. 720, n. 10). Yet if Fraser draws conclusions about the proliferation of public spheres that postmodern theorists might find congenial, Villa neglects to note that her comments are unmistakably motivated by the modernist agenda of publicity, equality, pluralism, and democratic self-determination. For a similar argument, see Young 1989 on the notion of a "heterogeneous public."

11. This comports with Habermas' view that the idealized conditions of discourse are "counterfactual" and his claims about the limits of social theory (1984, 42; 1992, 60, 92–93, 160–61, 168, 180). In fairness, Villa seems at times to countenance precisely this reading of the public sphere (pp. 712–13); but he never fully articulates his views. Moreover, he offers a partial reading of this passage from Foucault that belies such an interpretation of his position (p. 719, n. 2).

12. This is not quite so easy as critical theorists might initially suppose, especially if they hope to confront squarely postmodern suspicions that communicative reason is irremediably "regularizing." Minimally, it would require that they disentangle a set of issues that Habermas' own language tends to obscure. For example, reaching *understanding* (according to Habermas, the implicit telos of language use) does not in any straightforward way entail *consent* (according to Habermas, the mechanism by which speech acts coordinate social interaction). Nor need it issue in *consensus* or *agreement*. Shared understandings may, in fact, ground profound disagreement.

13. Villa's predicament is even starker than MacIntyre's insofar as he provides no criteria for determining which political "attitude" one ought to adopt when confronted by the "choice" he offers.

14. This is so even in Villa's lengthy final footnote. Referring to Fraser (1992), he admits—but does not explore—the possibility that our predicament might not be quite so dire as he makes out, that there may be alternatives other than "the purely mournful or subversive attitudes" he acknowledges in the text. But he also asserts without argument that the notion of the public sphere upon which such an alternative rests "ceases to function in an essentially normative capacity" (p.

720, n. 10). This view reflects Villa's failure, noted earlier, to recognize the essentially modernist impulses that inform Fraser's criticism of Habermas.

15. Here, as with Foucault, Villa is ill served by conventional interpretations of texts.

16. See Johnson 1991 on the difficulties Habermas has in treating strategic action. In any case, insofar as postmodern theorists include expressive and aesthetic as well as strategic dimensions in "agonistic" relations, what I depict as an opening is just that—a potential beginning, an invitation. Here, Habermas' cryptic discussion of "dramaturgical action" with its roots in Goffman's (1959) analysis of "the presentation of self" might afford still another point of departure for discussion between postmodernists and critical theorists (Habermas 1984, 90–94). For Lyotard specifically cites Goffman among the sociologists who contribute to our understanding of the agonistic dimensions of social relations (1984, 16–17, n. 60). This conversation might explore such issues as whether (and when) expressive actions really are, as Villa claims (p. 718) and Goffman denies, nonteleological.

17. Habermas views the debate between postmodernists and critical theorists in terms that mirror those that Villa poses. As Habermas remarks: "The contrast between rationalist approaches in Germany and post-structuralist approaches in France is being quite unnecessarily exaggerated at the moment. . . . I can scarcely recognize myself in the caricatured image of my theory that is sometimes drawn in this dispute" (1990b, 127).

18. For instance, he fails to acknowledge White (1991) and McCarthy (1991); but see Villa 1993. In any case, from reading Villa, one would never guess that Habermas does not fully and unambivalently embrace modernity, that he considers it a precarious achievement and that he is critical of the one-sided (hence incomplete) way in which it has developed.

19. This is not simply an idiosyncratic predilection: "A whole morality is at stake, the morality that concerns the search for truth and the relation to the other" (Foucault 1984, 381). It nevertheless goes without saying that discussion can be frank, pointed, even heated and that it can be enlightening without issuing in agreement.

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FORTHCOMING IN SEPTEMBER, 1994

The following articles, research notes, and controversies have been scheduled for publication in a forthcoming issue.

William James Booth, "On the Idea of the Moral Economy."

Gary W. Cox. "Strategic Voting Equilibria under the Single Non-transferable Vote."

Mary G. Dietz. "The Slow Boring of Hard Boards: Methodical Thinking and the Work of Politics."

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POLITICAL THEORY

Democracy and Decision: The Pure Theory of Electoral Preference. By Geoffrey Brennan and Loren Lomasky. New York: Cambridge University Press, 1993. 237p. $44.95.

Geoffrey Brennan has coauthored, with James Buchanan, a seminal work in constitutional political economy, *The Reason of Rules,* and *The Power To Tax.* Loren Lomasky is a philosopher interested in questions of authority and rights. After previously coediting a book, *Politics and Process,* together, they have now jointly written an important new book, *Democracy and Decision,* which is a critique of public choice theory as it applies to the electoral arena.

At first blush, this is the best of times for public choice theory and its political science alter ego, positive political theory. With a Nobel Prize in economics to leading practitioner James Buchanan, public choice gained added legitimacy in economics, while in political science, theorem-laden articles appear in the discipline's leading journals with regularity. In the Twenty-Fifth Anniversary edition of *Public Choice,* Dennis Mueller, author of the leading public choice textbook, opines, "It is difficult to believe that political science will not be taken over by formal modeling of rational actors' behavior in the same way that economics has been. Inductive theorizing invariably loses out to deductive; less formal modeling invariably loses out to more formal modeling" (77[1993]: 146).

But all is not roses for rational choice modelers. In political science, the rise of rational choice modeling has led to a reaction of epic proportions, similar in tone to the antibehavioral movement of the late fifties and early sixties and directly comparable to the (unsuccessful) rearguard fight of older economists against the increased use of mathematics in that profession after World War II. According to Mueller, "While [that] takeover was occurring, complaints could be heard that the new techniques 'were not economics', were being used 'to reinvent the wheel', were 'divorced from reality', and the like" (ibid.): by simply substituting *political science* for *economics* in this quote, we obtain the basic argument of the current rational-choice bashing repertoire in political science. All that we need to add is the common accusation that public choice is just a cover for right-wing ideas.

If anti-public-choice sentiment were confined to political scientists of a humanist (or left-wing) persuasion, we might be able to dismiss it as simply old-fogeyism or leftist paranoia; but concern about the lack of realism and lack of explanatory power of rational choice models is widespread and is found even among practitioners of public choice and positive political theory. In particular, there has arisen a concern that public choice models in their purest form (i.e., ones based on a narrow definition of self-interest or a monomaniacal focus on a single goal such as electoral victory) simply miss too many important features of politics. Thus, we have Morris Fiorina titling a 1991 paper "Is Turnout the Paradox That Ate Rational Choice Theory?," while William Niskanen goes even further, suggesting that although public choice has become a recognized field of academic study, "it is not clear that we have much to tell the rest of the world." Niskanen then goes on to note that public choice theorists not only cannot answer such questions as "Why do people vote?" but "have no satisfactory explanations of the major political developments of our lifetime: the massive expansion of government budgets and regulation, the erosion of the economic constitution, and the progressive spread and recent collapse of communism" (*Public Choice* 77(1993):155).

Democracy and Decision is reflective of this critique of public choice from within the subfield. While critical of public choice theories of voter behavior, it does not in any way reject the standard tools of microeconomics and applied decision theory, and, like Howard Margolis' work on altruism, it offers new models to help account for findings that appear largely inexplicable within a framework of pure self-interest narrowly defined. The central contribution of *Democracy and Decision* is the working out of a theory of *expressive voting,* whereby Brennan and Lomasky mean voting behavior that is not connected to an impact on outcome—like cheering for a team while watching them on television, even when you are alone at home.

My economist colleague Ami Glazer, also an exponent of an expressive model of voting, has suggested that "the ballot box is the last place where it is still legal to be a racist." Brennan and Lomasky argue that the ballot box is also a place where it is not particularly costly to be an altruist; that is, they turn the "selfish voter" argument on its head, by pointing out that because a voter is unlikely to be decisive, voters are much freer to vote in a way that makes themselves feel good by enhancing their self-image as ethical, principled beings. The argument is made nontautological by the assertion "All other things being equal, the relative significance of expressive elements increases by a factor equal to the inverse of the probability of being decisive" (p. 24). Thus, Brennan and Lomasky are able to say that *homo economicus* is generally a reasonable model for decisions about personal consumption but is, nonetheless, a bad model for choices in the public arena. Just as recent work by Donald Wittman (on competition for voters) or Norman Schofield (on cabinet formation) models parties as caring about both ideology and about winning, the Brennan and Lomasky book shows that allowing for expressive forms of voter preference and for multiple goals need not mean rejecting the value of formal modeling or of a rational choice approach, broadly speaking.

Brennan and Lomasky are able to trace both empirical and normative implications of viewing politics in terms of expressive choices. Normatively, they note that preferences revealed at the ballot box will not have the same claims to authenticity in terms of what economists call "demand revelation" as do preferences about private goods expressed through market choice. Empirically, they suggest how the expressive voting model can help account for a wide range of phenomena—from why facts and allegations about the private lives of public figures weigh into voter decision making to why public attitudes toward Social Security hinge more on perceptions of fairness than on the incidence of costs and benefits across generations.

There is no such thing as *the* rational choice model of any given phenomena, only *a* rational choice model. For example, even though the minimal winning coalition model does not work well in most countries in explaining cabinet coalition formation, this is no reason to give up on rational choice models of coalition building. The usefulness of any given model—rational choice or other—must be in terms of the insights it provides us to the particular phenomena we are seeking to understand and explain. Informed by common sense and focused on the substance of electoral politics, *Democracy and Decision* carefully steers between the Scylla of abstract rational-choice bashing without the offering of superior alternatives and the Charybdis of confusing degree of technical elegance or mathematical sophistication with likely social science importance.

Whatever your methodological stance, if you are interested in electoral politics, this is a book worth reading. Unlike much positive political theory, sometimes seemingly intentionally written *up* so as to remain incomprehensible to most political scientists, *Democracy and Decision* is lively and well written. Because the ideas in it suggest potentially testable alternative models in so many substantive domains, it is also a great book to give to graduate students looking for thesis topics.

University of California, Irvine BERNARD GROFMAN

Hannah Arendt: A Reinterpretation of Her Political Thought. By Margaret Canovan. New York: Cambridge University Press, 1992. 298p. $54.95.

Arendt, Camus, and Modern Rebellion. By Jeffrey C. Issac. New Haven: Yale University Press, 1992. 320p. $30.00.

Arendt was a theorist of the extraordinary, a philosophical poet of the preternatural: Holocaust and Achilles, concentration camps and ancient spaces of appearance, imperialism and revolutions in search of freedom. In these jarring historical and aesthetic phenomena, she located the massive dangers, as well as the possibilities, in the contemporary human condition. Future politics, in all of its possible senses, would only rise from the ashes of these multiple pasts: the brilliantly human and the hideously inhuman. No other future was an offer.

Arendt's political theory is thus not only intrinsically intriguing but precisely pertinent to many of our concerns and dilemmas. Fully cognizant of this, Canovan and Issac strive hard to unravel her complex theory and connect it to our politics. Yet their distinct readings of Arendt, interesting and suggestive, are too flawed substantively to be fully persuasive. In this sense, neither is cogent as an argument, but Canovan's study is far less compelling than Issac's in its analytic reach and accomplishments.

Long and detailed beyond the call of scholarly duty, Canovan's inquiry is *not* an argument about Arendt or a reinterpretation of her thought: it is an extended commentary on nearly all of her writings based on her assumption that Arendt has been "little understood" (p. 2). Two further claims buttress this one: that the "proper context" of her corpus is totalitarianism (p. 7) and that her real political theory lies hidden in her *unpublished* writings. No one, according to the intrepid Ms. Canovan, has really got Arendt right.

That Arendt's political theory is rooted in her encounter and engagement with the totalitarian Holocaust is not a novel theme. Kateb and many other scholars have not only insisted on this for over a decade, they have also analyzed the nuanced pathways that connect her theory and that terrible event. Canovan's stress on this point is puzzling and disingenuous. Far more troubling, however, is Canovan's doting reliance on Arendt's unpublished papers and her surprisingly uncritical attitude to them.

To Arendt, the private was usually a synonym for fantasy and falsity, a marker for those things not worth revealing to the public. Hence, a scholar who resorts to—and structures a "reinterpretation" (in fact, a commentary) on—Arendt's private, unpublished papers is under some obligation to justify her project. Canovan, however, does not, although she offers the feeble rationale that Arendt's thought is actually "inward-looking" and "esoteric" (pp. 11–12)—contrary to what Arendt herself has said and many interpreters have supposed. No evidence exists to support this outlandish claim, and none is adduced.

Nevertheless, the result of this strategy is ironic: the "insights" that she elicits from the private papers fail to shed any new light on Arendt's political theory. For example, Homer's pivotal role in Arendt's articulation of the nobility of action on which Canovan insists, is plain and patent in the *Human Condition* and in the extant published literature on Arendt. In fact, many of Canovan's breathless, analogous discoveries, based on her reading of the unpublished papers, fall into the familiar category. At best, they signal slight shifts in emphasis and occasionally add clarity, but they do not warrant a radical revision of her thought.

No novel interpretation or substantive reinterpretation of Arendt's political theory is visible in this study. Tracing the "sources" of a range of Arendt's ideas and commenting at length on them is apparently her forté. Canovan is instructive and often illuminating in her discussion of totalitarianism, barbarism and civilization, republicanism, citizenship, human plurality, and the relationship between philosophy and politics, although her tone is frequently cloying and defensive.

This peculiar style of intellectual empathy, in fact, accounts for a number of unusual but untenable claims. In her unlimited passion to "understand," Canovan zealously overreads and misconstrues Arendt's critical reflections. Two morally troubling examples should suffice. First, Canovan resuscitates the hoary canard that Arendt blamed the Jews for the terrible destruction they suffered at the hands of the Nazis (p. 44). (The only interesting question here is why she has revived this accusation.) Canovan asserts that in Arendt's analysis, "the Holocaust could never have happened" if only the European Jews "had a clear grasp of political principles" (p. 55). Not only did Arendt say nothing of the sort, but such a claim would contradict her unceasing theoretical emphasis on the radical contingency of life and politics. No credible textual evidence is produced by Canovan to support either indictment.

No less troubling is her rush to defend Arendt's solicitous analysis of white racist conduct in Africa and characterization of black natives as "savages." Canovan neither objects to, nor avoids, this label in discussing black suffering: on the contrary, she fluently explains it away as a corollary of their natural inferiority, their barbaric "worldlessness" (quoting Arendt). Canovan is not deterred, by the racist undertow in this analysis, from depicting Arendt as a ranking theorist of universal human rights.

Habitually uncritical commentary of this kind is not an accidental facet of Canovan's study: it has its source in her lack of theoretical savvy—in her unwillingness to fashion a standpoint from which critically to assess Arendt. Canovan casually assumes what she should be explaining in many cases—for instance, that Arendt is an antifoundationalist and an existentialist. In *Hannah Arendt,* Canovan succumbs to Arendt's considerable intellectual powers precisely because of this lacuna in Canovan's scholarly template, and the result is a study that is continually struggling to justify itself.

In contrast, Jeffrey Issac has a rough thesis in mind—that Arendt and Camus are theorists of "rebellious politics" but are rebels of a peculiar kind fitted for our postmodern time, suspicious of absolutes and final answers. Neither is duped by the boundless idealism of the Left, and both are protagonists of limits and moral realism. Hence, they are rebels who offer no "solace" and no lasting solutions. As Issac reads them, these "rebels" are not conspicuously or overly rebellious: they are, in fact, pliant liberals with a taste for the kind of sly pragmatism espoused by "leftists" like Rorty and Walzer.

Not only that, but they are both committed to "the project of democratic empowerment" and universal human rights (p. 242). Camus, possibly, fits this image of the prosaic modernist fairly well, but to cast Arendt into this mold is to stretch credulity. Focusing primarily on Camus, Issac has fashioned an interpretive net that *can* accommodate Arendt, but at her expense. To stuff Arendt into the Enlightenment straightjacket is badly to misread her, and Issac implicitly recognizes this: he frequently retreats from his sanguine assessments of Arendt as an American liberal.

But there is a deeper problem with this interpretation—the presumption that Camus and Arendt were intellectual equals. His literary acumen notwithstanding, Camus was hardly "an exemplary theorist" or even a "political theorist" in any precise, substantive sense (see pp. 11–12). An interest in similar themes was the legacy of a shared historical and philosophical milieu, not a sign of intellectual parity. Camus and Arendt were substantively incompatible figures: he was intellectually light, a parasitic man of letters who said a few interesting things; she was an extraordinary, original theorist of the post-Holocaust human condition, an epic thinker who uncovered new trails of thought.

This reductive strategy not only trivializes Arendt, it robs her analysis of its radicalism and reduces it to the level of Camus's trite bourgeois formulas. For instance, Arendt condemned the racism and unwillingness of Zionist leaders to make peace with the Palestinian natives. In a typically Camusian move, Issac equates this critique with Camus's staunch opposition to independence for Algeria. Camus's insolent dismissal of the colonized is a far cry from Arendt's strikingly unbiased judgment against the oppression of the Palestinians by the incoming European Jews.

Walzer's tutelege is very much in evidence in Issac's analysis. Issac's adulation of Camus and praise for his "compelling" reasons for rejecting Algerian claims (p. 205) and Issac's staging of Arendt as a theorist of limits, a pragmatist wary of utopianism, and an enthusiast of "civil society" are variations on the Walzerian passion for lukewarm liberal politics, "connected critics," and ethnic affiliation. That Arendt can be read selectively in this way is not in dispute. The crucial point, rather, is that this Walzerian reading traduces her brilliance and the unique radicalism of her political theory. For compromising politics of this sort, especially the Camusian variety, Arendt had no sympathy. To say that she "misunderstood his work" is an untenable assertion (see p. 16). On the contrary, Arendt understood precisely the bourgeois temper of Camus's mind.

Still, Issac's study is a provocative, interesting *interpretation,* even though he speculates well beyond the spacious bounds of Arendt's political theory. Canovan's book, on the other hand, is a profound intellectual disappointment, an immense effort that goes nowhere. Had Canovan attempted to do what, in the final pages of her faltering conclusion, she recognized she should have, she would have produced a commanding text—an interpretation of Arendt based on a "thorough assessment of the kind that cannot be attempted here" (p. 280).

St. Francis Xavier University SHIRAZ DOSSA

The Idea of Democracy. Edited by David Copp, Jean Hampton, and John E. Roemer. New York: Cambridge University Press, 1993. 449p. $59.95.

This collection consists of five parts. Part 1 concerns the "point" of democracy; parts 2–4 relate democracy to "preferences," "public reason," and "economics"; and part 5 consists of cases studies. Each part contains main papers plus commentaries, with contributors from a number of different disciplines. However, the editors more or less frankly acknowledge that these divisions are procrustean and arbitrary, calling attention to the overlap between discussions in different parts. If anything, the parts correspond to different theoretical approaches, rather than dealing with separate issues. For example, the appropriateness of industrial democracy arises largely as a moral issue in part 1 (Arneson) and largely as an economic issue in part 4 (Bowles and Gintis).

Two implicit foci of debate are consequentialist-versus-nonconsequentialist justifications of democracy and the role of preferences. In relation to the first focus, Estlund considers, in a challenging but rather obscure piece, the idea that if democracy is justified by its consequences and if there are political experts, we should defer to them and so undermine democracy. His preferred response is to question whether such experts could ever be recognized and therefore whether such deference could ever be rational. Copp, in his commentary, argues that a form of government has to be judged not merely on how well it deals with social decisions but also on what sort of system it is. But this, he insists, is not to depart from the terrain of consequentialism. However, he fails to consider a familiar point that does involve such a departure and does create problems. Democracy may be valued not because it *fosters* autonomy (consequentialist) but because, being a system of self-rule, it necessarily instantiates it (intrinsic). The instantiation of autonomy may then be accompanied by a very poor performance in terms of the outcome of its social decisions. When Copp says, "We value democracy because we think democracy will have the best consequences for society, all things considered, and because of the kind of governance it is" (p. 115), we have to recognize that these two things may fall apart, forcing us to decide which we regard as more important.

Preferences get discussed in a number of ways. In a general discussion, Hardin rehearses the problems in aggregating preferences recognized by Arrow and motivational difficulties connected with the free-rider problem. He argues that the same assumptions that have done wonders in explicating economic activity have exposed grievous flaws in democratic thought and practice. Christiano argues that there is an intrinsic justification for democratic control of public-collective properties, as opposed to mere preferences, which are not necessarily expressed in the act of voting anyway—thus attempting to short-circuit the Arrow problem. As for the free-rider problem, Hardin does not consider the possibility that while the requisite assumptions may be true of human beings, they may not be *all* that is true of them: if human beings are not *exclusively* self-interested agents, it may be that there are other features of their makeup that can explain and justify other forms of behavior than the economic. Sunstein enters a robust case for democratic action against people's preferences if they have resulted from unjust background conditions. He gives a long list of possible instances where state intervention might be justified but leaves matters at the level of moral intuition, rather than going on to provide reasoned justifications for them.

Where actual preferences are in some substantive sense not arrived at autonomously, the so-called paradox of emancipation arises: if you think people express preferences at odds with their own interests but you also wish to abide by democratic procedures, do you allow actual preferences to determine a suboptimal outcome or override their preferences and therefore apparently abandon democratic procedures? That is a question usefully raised by a number of these papers.

Answering it might involve raising further questions about what most of them take for granted—broadly, the fabric of existing, representative democracy. There has been interesting recent work laying down a challenge to this fabric and mooting the possibility of more participatory institutions (e.g., Fishkin's *Democracy and Deliberation,* 1991), but it is not discussed here. In addition, although the editors refer to the move to democracy in Eastern Europe, there is no indication that anything of theoretical importance can be learnt from that ghastly failed experiment. Roemer, it is true, urges that we should separate out the factors responsible for that failure and not just assume that one-party rule *and* a command economy *and* state ownership are equally hopeless; but his paper is concerned with the feasibility of state control of the commanding heights of the economy, rather than with the political issues. So far as democracy is concerned, it is the political component in that failure—the *vanguardism* with which Leninist doctrine answers the paradox of emancipation—that requires examination. Even more recent events in Eastern Europe should warn us against the thought that once indefensible doctrines have been seen off, they are never heard of again.

The collection, then, contains many interesting discussions but possesses little unity. Rawls's paper, "The Domain of the Political and Overlapping Consensus," for example, is reprinted and elicits an eloquent response from Joshua Cohen, who attempts to show that Rawls's later thinking does not lead to an abandonment of his earlier egalitarianism, as well as a penetrating critique from Hampton designed to show that defensible liberalism cannot really be neutral. But Rawls's project *presupposes* a democratic political culture, rather than being centrally concerned with its justification.

Finally, any reviewer should cavil at one aspect of the production of this volume. Where articles have been published before, commentators generally refer to the pagination of the original (and in one case to the pagination of a presumably unavailable conference typescript), rather than that in the present volume. That does not facilitate scholarly enquiry.

University of Bristol KEITH GRAHAM

The Other Heidegger. By Fred Dallmayr. Ithaca: Cornell University Press, 1993. 232p. $30.75.

Heidegger's Crisis: Philosophy and Politics in Nazi Germany. By Hans Sluga. Cambridge: Harvard University Press, 1993. 285p. $29.95.

The common thread connecting these two useful books is the vexed problem of the relation between Heidegger's philosophy and his nazism. Heidegger joined the Nazi party and served as the Nazi rector of the University of Freiburg briefly in 1933–34 before resigning. The relation between his philosophical theory and his political turning is the subject of much debate, particularly since the publication of Victor Farías' book, *Heidegger and Nazism,* in French in 1987.

Since that time, there has been a steady stream of books in French and English devoted to aspects of the link between Heidegger's theory and his nazism. The present volumes respond to the continuing controversy. In 'The Rectorate 1933/34: Facts and Thoughts"—an article written in 1945 that only appeared in 1983—Heidegger contended that his brush with nazism was essentially meaningless and certainly so for an evaluation of his thought. Taking a similar line, Dallmayr and Sluga both acknowledge Heidegger's nazism but seek to relativize it in a way that leaves the wider theory untouched (or at least relatively so) by his political turning. Dallmayr identifies another Heidegger who is a radical critic of just what he seems to have been identified with, and Sluga shows that Heidegger's comportment was rather typical of what passed for responsible academic behavior during a rather difficult period.

I am impressed by Sluga's careful account of the philosophical background in which Heidegger turned to nazism and Dallmayr's equally careful interpretation of Heideggerian texts. Both of these books are important contributions to the continuing effort to comprehend the link between Heidegger's nazism and his philosophy. Yet I find their respective efforts to explain away (or at least severely to weaken) the importance of Heidegger's link to nazism for a grasp of his thought to be unpersuasive. As a player in the game, I believe that Heidegger's nazism is finally inseparable from his theory. Accordingly, my remarks on specific points in Sluga's and Dallmayr's books are meant to reflect my unease about their respective interpretative strategies.

Sluga's contextualist approach to Heidegger continues a tendency that has recently been featured in publications by Ott, Nolte, Farías, Bernd Martin, Thomas Laugstien, George Leaman, Claudia Schorcht, Rainer Marten, and others. He presents the most detailed view we currently possess of German philosophy in general during this terrible period. Whereas others look to the historical context to reveal the depths of the problem, he relies on a grasp of the philosophical context to show how typical Heidegger was. To make his case, Sluga talks comparatively little about Heidegger but a great deal about the surrounding view of well-known and comparatively obscure German thinkers of the period. Even if few think that talk of the good actually leads to doing the good, the extent to which German philosophers rushed to cooperate with nazism—in effect, to conduct business as usual in a changing environment—is clearly disturbing. Sluga's discussion is fascinating and sobering. He largely succeeds in his effort "to embed the story of Heidegger's personal crisis in an account of the crisis that enveloped German philosophy in general" (pp. viii–ix).

Both Sluga and Dallmayr prefer not to engage alternative ways of reading the link between Heidegger's thought and his nazism. Sluga regards the prior debate as simply factional, moralistic, and individualistic and psychologizing. Bourdieu receives special discussion early in the book for a simplified account of the philosophical context and later on for a supposedly reductionist approach (pp. 9–10, 247–48). Sluga argues in detail that Heidegger was not unique, since many other German philosophers turned to Nazi politics. His accounts of Heyse and Bauch are especially useful. He further innovates in two other ways. One is his careful discussion of Fichte, whose theory was appropriated by the Nazis as part of the intellectual basis of national socialism. The other is his insistance on the Nazi period as a time of crisis and the perceived crisis as a motivating factor in the widespread philosophical turning to nazism.

The effort to incriminate Fichte as a kind of proto-nazi has been made by others, recently by Glucksmann (*Les maîtres penseurs*), although perhaps never through such detailed comparison. Yet it is unclear that Fichte's situation in 1807 and that of German nazism in the twentieth century is even roughly similar. The view of the Weimar Republic as a time of crisis is prominent in Heidegger's 1929/30 winter lecture series (*Die Grundbegriffe der Metaphysik: Welt, Endlichkeit, Einsamkeit*). Despite Nolte's urging, the fact nazism occurred in a time of crisis seems to me to be insufficient either to excuse the general academic turning to nazism or to excuse or otherwise minimize Heidegger's political turning.

Sluga's Heidegger is too much like the other thinkers of the period. We need to recall that Heidegger was the only philosopher of the first rank to rally to the Nazi cause. Among German philosophers, he was committed unusually early and to an unusual extent to national socialism. Sluga insists (correctly, in my view) that Heidegger was engaged in self-promotion (see p. 173), yet this is an instance of the psychologizing tendency that Sluga rejects. So, too, is the claim that Heidegger's nazi turning is due to his desire for his theory to have practical effects (see p. 231). For Sluga, political ideas reflect political conditions (p. 246). What about philosophical ideas in general? Is this the case for Heidegger? Did Heidegger's theory lead him to nazism?

Dallmayr's book, which consists of a series of essays (some previously published) is rightly seen as part of the current effort by Derrida, Lacoue-Labarthe, Caputo, and others to acknowledge Heidegger's nazism while recovering the still-valid part of the theory. For Dallmayr, the fascist element in Heidegger's thought is "*one* (easily the most deplorable) strand, but not the only and not even the dominant one" (p. ix). To put the point bluntly but not inaccurately, breaking with the view of Heidegger as a good philosopher but a bad politician, for Dallmayr, Heidegger is a good philosopher and a good politician as well, although in a theoretical, not a practical, sense.

Dallmayr's discussion is invariably based on sensitive, generous, and occasionally overgenerous interpretation of the full range of Heidegger's writings, with special attention to the later thought. The frequent remarks on the *Beiträge zur Philosophie,* a still little known but central work of Heidegger's later thought, are particularly useful. Four chapters (roughly half the volume) are devoted to aspects of Heidegger and politics; the remainder ranges widely over such topics as ethics and justice, otherness, friendship, and Nishitani's version of zen Buddhism.

The effort to reveal another possible non-fascist interpretation of Heidegger's theory is plausible, since few commentators, other than Adorno, want to "reduce" Heidegger's theory to his nazi proclivities. Yet Dallmayr, who confesses to being awed by Heidegger (see p. ix), could have strengthened his case for another Heidegger by taking on the views of those who disagree with him. In some instances, such as the *Letter on Humanism,* his readings are vastly undetermined by the facts—as if Dallmayr regarded as obviously true Heidegger's own, unsupported claim that he later turned against national socialism.

The four chapters on Heidegger and politics depict a Heidegger as subversive of the established order, which is perhaps less at odds with his nazism than Dallmayr pretends. He holds that after 1933, Heidegger's corpus reflects a profound struggle against nazism without returning to traditional orthodoxy (see p. 19). But Heidegger did not even resign as rector until the middle of 1934. Dallmayr, who discusses the views of Pöggeler and Derrida, does not bolster his conviction through textual evidence. An important example is the reference to Heidegger's rejection of the nazi race theories, which is routinely used by his defenders to argue that he was not a Nazi. Dallmayr's true remark that Heidegger was unable to support "naturalism" or "biologism", does not justify his surprising conclusion that "the critique of modern ideologies (deriving from metaphysics) cannot possibly entail support for Hitlerism or National Socialism" (p. 47). Many German philosophers, including Heidegger, who were unenthusiastic about (and even clearly opposed to) such doctrines, supported nazism. Although Dallmayr reads the *Letter on Humanism* as opposing nazism (see p. 63), it can more plausibly be read as an effort at political rehabilitation at a time when Heidegger still held to his nazi convictions. It is correct that Heidegger was opposed to popular sovereignty (p. 104), although one misses an analysis of the Platonic nature of this claim. Dallmayr reads the first series of Hölderlin lectures as moving away from nazism, although he fails to consider the residue of folk (völkische) ideology in this document.

I share Dallmayr's conviction that Heidegger's theory is "suffused with ethical preoccupations" (p. 109). He regards Heidegger's resistance to formalized ethical theory as related to a rejection of nazism as a racial-biological theory (pp. 125, 130). He further regards Heidegger's rejection of "racial-biological chauvinism" as tantamount to the effort to choose an alternative course (p. 152), yet the texts lend insufficient support to this hypothesis. In the same way, Heidegger's traditional German philosophical emphasis on the renewal of ancient Greece in the first series of Hölderlin lectures (p. 176) can also be read as linking his longing for Hellas with his nazism. The discussion of friendship is devoted to an uncontroversial account of Hebel. The salute to Nishitani emphasizes his concern with Western philosophy but fails to consider the link between his views and the ambiguous link of his teacher, Nishida, to fascism.

Duquesne University Tom Rockmore

American Democracy: Aspects of Practical Liberalism. By Gottfried Dietze. John Hopkins University. Baltimore: Johns Hopkins University Press, 1993. 290p. $39.95.

Self-consciously, this book is in the tradition of Tocqueville, Bryce, and Laski. Its author, Gottfried Dietze, an erudite scholar and renowned teacher, is a native of the land of Beethoven, Hegel, and Goethe. He is also a long-time American resident and observer of the American people, especially their politics. Unfortunately, this book is less than we might have hoped.

It is not a closely argued book: the style is soft and remarkably, (sometimes almost randomly) discursive. For example, he writes: "Freedom does not, as Spinoza believed, produce just good. Following the Libson earthquake of 1755, an event that considerably muted the optimism of the time, the dangers of liberty were pointed out by the authors of the *Federalist Papers* and by Kant, who followed thoughts expressed earlier by Hobbes, Locke, and Blackstone" (p.19). The oddity of this sentence lies not only in the connection that it suggests should be made between a natural event and the political views of a wide array of philosophers, two of whom lived before it occurred. Even more strange is the implication that the authors of the *Federalist* in some significant way sided with Kant against Spinoza. Or is all this just rhetorical flourish?

Whatever it is, there is a lot of it in this book and mostly with uncertain effect. The author has a powerful tendency to scatter not only a wide array of little-explained references through his pages but also quotations, some extended. The general impression left certainly includes a burnish of imaginative erudition. But it also leaves one wondering once again whether arguments are just being loosely illustrated or specifically and authoritatively documented.

These difficulties carry over into the crucial problem of definitions. *Liberalism,* although much discussed (documented? illustrated?) is let go essentially as the desire for freedom, built into human nature and as old as man himself. *Classical liberalism,* a tradition with which the author identifies himself, is the same but restrained by law and principles of morality. And *democracy,* of which America is said to be the premier example, is passed off more than once as simply government by the people (or, at least, majority rule). The principal effect of this looseness in definition is to render the book's argument imprecise, its chapters unfocused, and even many of its paragraphs poorly structured.

Another effect is make many of the books remarks appear to be not just asides but the opinionated views of an unrepentant "conservative," especially on subjects as diverse as modern art, the morality of American youth and popular culture, and the influence on modern times of Freud, Nietzsche, and Marx. It is important to mention this aspect of the book because of the possibility that other readers, including especially those who share most of Dietze's opinions, might find this book wide-ranging, insightful, and articulate. Even in the face of that possibility, I found Dietze's central thesis unsatisfying. That thesis, as repeatedly stated, is simply that modern-day Americans, driven by primordial needs, history, and the unrivaled characteristics of their current circumstances, have dangerously strayed far from their classical liberal roots toward a pure liberalism that preaches an absolute freedom to do whatever one wants. We have become a consumer-oriented society in the broadest sense, free to make money, move, change religions, and accumulate both things and experiences just as we please and without limit.

In general terms, this is an unexceptional thesis that few on either the Right or the Left would dispute. But unless pinned down in terms of specific propositions and supported by both carefully sifted data and rigorous analysis, it leaves major questions hanging. In the context of this book and the possible perspectives of its author, two questions are especially significant.

First, for a book whose subtitle is *Aspects of Practical Liberalism,* is it enough to suggest that the liberalism of America's most practical document, its Constitution, lies in its being such a bundle of compromises that people (and politicians) are free to make of it as they please? Or would it be wiser to see the provisions of that document, (e.g., those establishing the Congress as an independent body) as embodying specific conceptions of the liberal ideology? Putting the matter another way, does our liberal constitution give us a government that can govern? By what means and to what standards? In the Germanic scholarly tradition, there are powerful voices that speak to questions of this order—most importantly, Max Weber's. It is our loss that Dietze did not use his advantages to give us a Weberian analysis of the American constitutional system.

Second, much of Dietze's analysis depends on a Hartzian perspective. Although he knows that perspective through and through, he gives us no critical analysis of it, much less an appraisal of its merits. But Hartz was notably deficient in not giving us much understanding of the content of the liberalism he found consensual in America, beyond saying it was an English liberalism. But is English liberalism different from German? More importantly, are there not German voices (most notably Hegel's in his treatment of civil society in *The Philosophy of Rights*) that can be profitably interpreted as supremely revealing and critical of English (American) liberalism? Again, it is to be regretted that Dietze did not make more use of his advantages.

New York University H. Mark Roelofs

Life's Dominion: An Argument about Abortion, Euthanasia, and Individual Freedom. By Ronald Dworkin. New York: Alfred A. Knopf, 1993. 273 p. $23.00.

That women have a "right to abortion" is a dogma of contemporary liberalism; that people have a "right to die" is well on its way to achieving dogmatic status. One is hardly surprised, therefore, to find Ronald Dworkin, a liberal moralist of the most impeccable orthodoxy, defending these putative rights in his new book on abortion and euthanasia.

What *is* surprising is Dworkin's claim that the debate about abortion and euthanasia reflects competing "interpretations" of a shared basic commitment. He suggests that those who favor abortion and euthanasia, no less than those who oppose them, believe in the intrinsic value—and, indeed, the sacredness or inviolability—of human life. Debate breaks out because the contending parties "interpret" life's value, sacredness, and inviolability differently.

For example, proponents of "abortion rights," far from denying the sanctity of human life, may, according to Dworkin, reasonably conclude "that abortion is sometimes necessary in order truly to respect life's inherent value, which depends on the intrinsic importance of human creative investment [in it]" (p. 157). For such people, as for proponents of euthanasia, the "life" that is sacred and inviolable is not "biological life" but the "human life [that] is created not just by divine and natural forces but also . . . by personal choice, training, commitment, and decision" (p. 93). They hold that "it may be more frustrating of life's miracle when an adult's ambitions, talents, training, and expectations are wasted because of an unforeseen and unwanted pregnancy than when a fetus dies before any significant investment of that kind has been made" (ibid.).

Dworkin devotes about two-thirds of *Life's Dominion* to a defense of the regime of abortion law put into place by the Supreme Court in *Roe* v. *Wade*. In certain respects, however, his account of the constitutional law of abortion is questionable. Apparently overlooking the implications of *Doe* v. *Bolton*, the companion case handed down with *Roe* on January 22, 1973, he implies that states may meaningfully protect the unborn against abortion in the final trimester of gestation. As Mary Ann Glendon has observed, however, that proposition is subject to the gravest doubt in view of the Court's holding, in *Doe*, that there is a constitutional right to abortion through the third trimester to protect maternal health and its expansive definition of *health* as including all factors, physical, emotional, and even "familial."

Dworkin maintains that the controversy over abortion, which continues to rage 20 years after the Supreme Court purported to settle the matter, "is based on a widespread intellectual confusion we can identify and dispel" (p. 10). He assures us that "once the confusion has been identified, we will see that a responsible legal settlement of the controversy, one that will not insult or demean any group, one that everyone can accept with full self-respect, is indeed available" (pp. 10–11).

What is the alleged confusion? Dworkin maintains that public debate largely fails to recognize a "crucial distinction" between objecting to abortion because it violates the right of unborn human beings not to be killed (the *derivative objection*) and objecting to abortion "because it disregards and insults the intrinsic value, the sacred character, of any stage or form of human life" (the *detached objection*) (p. 11). The derivative objection, he maintains, is unsound because human fetuses, nearly until birth, lack all "interests," including "an interest in surviving," and therefore cannot have a right not to be killed (pp. 18–19). The detached objection, by contrast, is not unreasonable. However, it merely provides grounds for "personal opposition" to abortion and, perhaps, efforts to make people who are considering abortions pause to reflect carefully on what is, after all, a serious moral decision; it does not warrant outlawing deliberate feticide.

Dworkin argues that most people who oppose abortion do so on reasonable (i.e., detached) grounds. The problem is that many of them fail to recognize that the detached objection does not justify laws protecting the unborn against abortion. Hence, they wrongly seek to impose their view on others who reasonably interpret the intrinsic and inviolable value of life as permitting—and, indeed, sometimes even requiring—abortion. If advocates of the pro-life cause would only recognize their confusion, Dworkin suggests, they could abandon the political struggle while retaining their commitment to the sanctity of life. *That*, it seems, is the "reasonable settlement" that Dworkin believes "all parties" can accept while retaining their self-respect. In effect, Dworkin offers the prolife movement what he takes to be honorable terms of surrender.

Anticipating a possible objection, Dworkin considers the fact that our law protects many nonhuman realities whose destruction or violation we object to on detached grounds. We protect the environment, for example, or endangered species not on the ground that they have rights but, rather, because we consider them inherently valuable. It is here that his redescription of the proabortion and proeuthanasia positions as expressing a competing "interpretation" of a shared basic commitment to the sanctity of life has its payoff: it enables Dworkin to cast the debate about abortion and euthanasia as essentially "religious" in character, that is, as a debate about "the meaning of life." Hence, according to Dworkin, the First Amendment's protection of the free exercise of religion forbids government from prohibiting what he bluntly concedes is the book's opening sentence are forms of "deliberate killing" and "choices for death" (p. 3).

The success of Dworkin's argument hinges on his distinction between detached and derivative objections to these death-dealing choices and on his claim that derivative objections to the killing of unborn and seriously demented human beings are unreasonable. These, in turn, depend on the validity of the person–body dualism implicit in Dworkin's argument.

Opponents of abortion and euthanasia reject the proposition that some human bodies are "inhabited" by persons, while others, such as bodies of unborn and gravely impaired human beings, are not. They do not believe that incorporeal persons inhabit nonpersonal bodies. Thus, they view all human beings, including the unborn and the demented, as members of the community of subjects to whom duties in justice are owed—that is to say, as *persons*, or (if you will), "rights-bearers."

Dworkin's case against the view actually held by sophisticated supporters of the pro-life position strikes me as woefully inadequate. His argument that fetuses cannot have "interests" and therefore have no right not to be killed, straightforwardly begs the question; and he makes no effort to respond to the critique of person–body dualism pressed against his position by, among others, his fellow Oxford professor of legal philosophy, John Finnis.

Moreover, his talk about the contending parties in the debates over abortion and euthanasia sharing a basic commitment to the sanctity of life and only interpreting this commitment differently, is, I believe, practically insignificant. The real issue of principle between supporters of abortion and euthanasia and opponents of these forms of killing has to do with the question of who are subjects of justice.

The challenge to the orthodox liberal view of abortion and euthanasia is to identify nonarbitrary grounds for holding that the unborn and seriously demented do not qualify as subjects of justice. Frankly, I doubt that this challenge can be met. In any event, Dworkin here fails to make much progress toward meeting it.

Princeton University ROBERT P. GEORGE

A New Constitutionalism: Designing Political Institutions for a Good Society. Edited by Stephen L. Elkin and Karol Edward Soltan. Chicago: University of Chicago Press, 1993. 240p. $45.00 cloth, $15.95 paper.

The essayists in this collection believe "that something systematic can be said about how political institutions work and can be made to work . . . and why some regimes are better than others' (p. viii). In the current intellectual climate, this seems an almost reckless statement, since postmodern analysis has quite powerfully suggested how tightly bound all political communities are to their particular and contingent rhetorical conditions. The subtitle might lead one to expect a comparative analysis of constitutional regimes rigorous enough to combat postmodernism's claim and to show that political research can escape the gravitational pull of particular cultures, find the philosopher's stone by which to show the superiority of one system's political values over another's, and thereby demonstrate the political correctness of particular constitutional designs.

There is, in fact, no serious cross-national analysis here, nor do we find a new constitutionalism through an analysis of different ways of constituting complex political regimes in general (e.g., Minnesota–Louisiana, University of Chicago–University of Georgia, IBM–Microsoft). Instead, readers will find here another critique (warranted, in my judgment) of contemporary political science. Readers will confront exhortations to abandon analyzing bits of political data, rhetorical styles, and analytic philosophy and, instead, to get on with the task of designing good national regimes.

These pages *do* contain many important ideas. Much postmodern thought does encourage the "hermeneutics of suspicion," and that attitude may plausibly corrode government. An ancient version of the same corroded the Roman Empire, according to Gibbon. Karol Soltan urges that designing good government means designing better citizen education to achieve greater sophistication about politics. Borrowing on Chomsky, Soltan wants the new constitutionalism to describe standards by which the ideal citizen could become an effective political speaker–hearer. Stephen Elkin draws from Kenneth Arrow the goal of designing a state that achieves the "proper aggregation of voter preferences" (p. 29). James Ceaser argues that rational choice analysis cannot respond effectively to value-laden cries for justice. Charles Anderson, restating the main themes of his book, shows pragmatism's compatibility with liberal values and goals. Theodore Lowi and Cass Sunstein deliver their familiar pitches elegantly: Lowi decries the delegation of legislative power to the executive, while Sunstein, less polemical than Lowi, points out that the founders's original hope that national government would play the role of public-spirited deliberation survives in the role played by the judiciary.

The book thus presents a theme: We must develop confidence in our capacity to define problems and their solutions through conversations that are "practical" and "conditionally normative" (p. 133). But experiencing this worthy theme-and-variations finally resembles nothing so much as the familiar pattern into which romantic relationships are said to fall. One begins with hope for something new and challenging and in due course finds oneself in the same, quite-familiar place with the same familiar comforts and the same unresolved tensions and unmet yearnings. Thus, in Edwin Haefele's concluding essay, we learn that the American republic is constituted by self-government, civic virtue, a regulated private economy, and separation of church and state.

Hence, the constitutionalism proposed here is hardly new. The authors urge us to return to the thrilling days of yesteryear, when people actually could talk confidently about designing good constitutions for good societies: it is a romantic book. In its romantic yearning for a kind of perfection, it does not convincingly demonstrate the existence of a constitutional design problem serious enough to call for major changes in the first place. In comparison with other regimes, our design does seem to have avoided police states. Our political system has not drastically limited human mobility. We are reasonably free of social-caste distinctions. The political system seems not to obstruct adapting business practices to the new global economy. Of course, public education is a mess, but the authors do not explain why we need a new constitutional design to clean that mess up.

It would also have helped if the collection somewhere addressed more aggressively the assumption that academic theorizing of this sort can ever translate into major political change. Civil wars and economic depressions may redesign constitutions, and in these crisis times, great thinkers help redesign, but this book does not coincide with a crisis.

Still, two things should be said on this book's behalf. First, Justice Holmes was quite right to point out that we need reeducation in the obvious more than elucidation of the obscure. Political scientists (because they are citizens, too) do have a responsibility for seeking knowledge that may better our political conditions. Precisely because postmodernism's criticisms are on target, conventional positivist/empirical political science must take stock of its political obligations. There is no justification for what we do beyond the expectation that we will contribute to betterment of life on the planet, and the authors rightly force us to examine whether our practices do so.

Second and more important, political change requires rhetorical change. This may be the subtextual agenda of the Committee on the Political Economy of the Good Society, from which these essays flow. Unfortunately, these essays admittedly do not offer the one thing rhetorical change requires, namely, a coherent doctrine of what the new constitutionalism should look like (p. 13). But by trying—and failing—to articulate a coherent theory of constitutional design among the classical and modern theories of political economy, the book helps invigorate the postmodern hope that truly new constitutional theories are worth creating. And these theories *will* (as this collection implies) address how we talk.

University of Georgia LEIF H. CARTER

The Dialogue of Justice: Toward a Self-Reflective Society. By James S. Fishkin, New Haven: Yale University Press, 1992. 243p. $27.50.

In *The Dialogue of Justice,* James Fishkin has produced a readable and stimulating approach to liberalism. Meta-level engagement with a range of contemporary liberal theorists leads up to a reworking and development of something close to the "dialogic" themes in Mill's *On Liberty* and an exploration of liberal society as "self-reflexive."

The book has three main parts. First, Fishkin argues that attempts at a systematic theory of the basis of a liberal political regime fail, because plausible ideas about what constitutes a just society pull irresolvably in conflicting directions. Second, he advances various plausible criteria, in light of which he criticizes other substantive viewpoints. Approaches to liberalism (1) must discriminate but not rule out everything, (2) should legitimize the state to all but not obligate citizens of other states, (3) should not provide other theories with a basis upon which to develop incompatible prescriptions, and (4) should not manipulate people's preferences. These ideas are brought alive in the critical discussion of other approaches, leading Fishkin to develop some useful typologies—for example, "brute," versus refined views of individuals' preferences, actual versus hypothetical situations, and neutrality versus universality. Fishkin's approach here is both interesting and powerful and displays the immense fruitfulness of general and meta-level argument, as contrasted with the pursuit of detailed points internal to the work of specific writers.

In the end, Fishkin finds the combination of these criteria too strong for any substantive theory. He then develops his own approach, by way of an exploration of the idea that preferences should not be coercively molded. He argues that political practices can justifiably claim support from all members of a society if (1) they are consensual in the sense of having wide support across major social cleavages, (2) are minimally voluntary (i.e., there is unimpeded exit, (3) essential benefits are supplied to all members of society, and (4) the society's practices are self-reflective, that is, subject to "continuing critical examination through unmediated debate" (p.). Fishkin develops these ideas in interesting ways, concluding with some practical suggestions about political institutions. Rather than paraphrasing his argument, which deserves to be read in detail, I shall move straight to some criticism.

First, nonmanipulation introduces more problems than Fishkin seems to allow for (see, e.g., problems raised in the community power debate and by the work of Foucault). Second, Fishkin appeals implicitly to dialogue-based ideas about epistemology. This is contentious both *qua* epistemology and also because it is incompatible with the implicit epistemological ideas of some of those who would be involved in his dialogue (e.g., fundamentalists). It is also not clear to what extent substantive ideas about benefits are being drawn from the epistemology as prerequisites to citizens' participation or are introduced extraneously. The latter would obviously be problematic; the former, to work, would depend on Fishkin's being able to argue that everyone can really make a potentially useful input to the argument.

Third, what one might call empowerment for the sake of participation is sometimes developed in ways that look more like the enhancement of pluralistic interest articulation, but this is by no means the same thing as dialogue. Dialogue itself introduces particular problems: it is not clear that people will be interested in listening to the arguments of others. Even today, serious discussion of political issues seems all too often to fall victim to the television remote-control zapper.

Fourth, the right of exit introduces a problem and highlights an ambiguity concerning the relation of dialogue and political community. Fishkin is working with the nation–state, but it is not clear why dialogue should generate or respect national boundaries. Why, say, should the views of those over the Mexican border (or in Australia) not count as much as those on this side? Is there a right of *entry*? Could the rich all "exit" and reconstitute their society free from the troublesome demands of the poor?

Fifth, there are also some interesting problems relating to criticism and political culture. Fishkin is himself ambiguous concerning of the position of religion at one point, arguing that it may underlie political culture (thus receiving protection but becoming subject to critical scrutiny) yet at another writing explicitly that it should be excluded from public dialogue (pp. 145, 202). (Clearly, exclusion means that in the case of conflict, what is public and subject to dialogue wins out, while religious views are treated as if they are matters of private taste; but it is not clear that believers should really want them treated in this way, not least because they will typically think that the content of their religious views is itself rationally compelling. Yet insofar as religious toleration and protection is the product of practical comprise, rather than of the principles that underlie the public sphere, this may itself mean that what is in the public sphere does not have the purely rational character that Fishkin's approach would require.)

In my view, one cannot insulate political culture from other fields; and given that political ideas will be strongly affected by other ideas (and vice versa), it is not clear that one should exclude them even if one could. All this, however, raises a more general problem concerning whether critical rationalism in the form of such openness to public scrutiny can sustain a popular political culture (see Ernest Gellner's *Postmodernism, Reason, and Religion*). Just as, in the earlier parts of Fishkin's book, one might well imagine that critical dialogue could take apart all positive ideas on the basis of which we might conduct our public lives, leaving nothing viable in their place. Or—to put it the other way round—is it not possible that the conduct of public life may require a certain measure of myth, ambiguity, and wishful thinking?

There also seem to me possible problems with Fishkin's concern for popular and unmediated, rather than merely public, self-reflection. Some of those who, like Hayek, defend a liberal economic order as the best for mankind also argue plausibly that it has certain unavoidable defects. The problem here is that *if* Hayek is right, the connections between the defects and the advantages (and hence the limitations upon remedying prima facie defects) are not easy to explain, and it is difficult to see how they could easily be legitimated to a society's non-theoretically-minded members.

That it raises such problems seems to me an indication of the interest and fruitfulness of Fishkin's approach, rather than a point against it. The book as a whole is readable, interesting, and very thought-provoking. Are there *any* defects? It does have a somewhat unfinished feel to it, and Fishkin's initial material could have done with more extensive introduction. His treatment of substantive issues could sometimes have done with a good deal more polishing; and Fishkin might usefully have returned, at the end of the volume, to discuss how his own approach related to the more general themes with which the book commenced. I suspect, however, that Fishkin will not be slow to address these and other issues that emerge from the present book, in a subsequent volume.

Australian National University JEREMY SCHEARMUR

Institutional Realism: Social and Political Constraints on Rational Actors. By Robert Grafstein. New Haven: Yale University Press, 1992. 244p. $30.00.

Robert Grafstein has made a valuable effort to confront fundamental questions about the ontological status of political institutions and their effects on political behavior. Grafstein's theory of institutional realism holds that institutions are "real things" that both limit and structure political behavior. He argues that institutions should be understood quite differently from conventions, roughly described as behavioral regularities due to common understanding. Institutions bring order and create patterns even when many people are not aware of the purposes that are being served. In short, the argument is that institutions can be treated as if they are exogenous when studying a good deal of political behavior. Institutions were not necessarily created for the purpose that they currently serve and participants may not be aware of the totality of the institution that constrains them.

Grafstein reached his position after a study of the conventional view, which is perhaps most widely known as it appears in social contract theory. In the state of nature, people recognize the dangers of anarchy and coalesce to create institutions under which they can live. He points out plenty of examples where scholars have interpreted regularized patterns of behavior (that is the way Grafstein defines institutions) as if they were conventions. For example, sociologist Talcott Parsons created an "overindividualized conception of society, one in which stable social structure must meaningfully mirror the standards and patterns of choice ascribed to individuals" (p. 43). Buchanan and Tullock are taken to task because they argue (in *The Calculus of Consent*) that constitutions are arrangements that are freely entered into as a refuge from the state of nature. Grafstein attacks the functionalist notions that institutions survive because they serve the desires of their participants or that institutions are a result of conscious agreements among people. Instead, "the institution in which agents are embedded establishes certain constraints on their behavior regardless of whether they are understood as such" (p. 82). The bulk of his argument against current spokespeople for rational choice theory (the "new institutionalists") is that they have not escaped the simplistic conceptions of the older conventionalism.

Two parts of Grafstein's argument are the most persuasive. First, if we rely simply on conventions, we do not have enough power to understand some institutions. We need to account for the fact that some institutions reproduce themselves without the conscious thought of their participants. Second, the conventionalist's tendency to interpret institutions as optimal communal adaptations should probably be abandoned. Some institutions persist despite their unpopularity. Institutions may be created and continue to exist even though the functions they serve are neither part of the intention of some of their original supporters nor of current participants. This anti-functionalist argument fits well within the current mainstream of political science thinking about institutions. Jack Knight's recent book, *Institutions and Social Conflict* (1992), advances the same argument. In his recent study of Congressional institutions (*Information and Legislative Organization*, 1991), Keith Krehbiel takes steps to avoid the functionalist tendencies in interpreting institutions, and attributes institutional design to human intention only when there is solid evidence for doing so.

Another, more dubious theme runs through Grafstein's book. He maintains that since humans are unaware of the institutions around them, our psychological models should be scaled back to reflect the limited nature of knowledge. While this claim probably does apply to some people (e.g., one-time visitors to a local school board meeting), it is hard to see how it can be consistently applied to fulltime politicians (e.g., members of the U.S. House of Representatives). Furthermore, by maintaining the argument that people are generally oblivious to institutions, Grafstein blocks himself from studying some of the really important questions about institutions such as "where do institutions come from?" and "why are some institutions more able to survive challenges than others?" One is reminded of the stance taken by Edward Sait who argued that "New social forms originate and old social forms die without any clear perception, by contemporaries, of what is happening" [*Political Institutions—A Preface* (1938, p.15)]. Sait maintained that institutions "seem to have been erected, almost like coral reefs, without conscious design." The extremely limiting nature of this theory of institutional creation and maintenance was a major part of the inspiration for the more modern theories of institutions that Grafstein is arguing against.

Instead of developing a theory of institution-building, Grafstein explores some ideas about the effects that institutions might have when they do exist. The most thoroughly developed example of this concerns voter turnout. Rational choice scholars have wrestled with the paradox of not voting for some time, but Grafstein finds their analysis wanting. He invokes evidential decision theory to assert that because of their institutional roles, people might perceive "an unavoidable stochastic relation between their behavior and the behavior of others" (p. 102). For some reason, people believe that if they vote, then others are more likely to do so. The mathematical model indicates that a rational person, guided by evidential decision theory, might choose to participate. Empirical details on the extent to which people actually do perceive interdependencies will need to be demonstrated in future research. In addition, the role of electoral institutions in causing these interdependencies to develop (and strengthen or weaken) will need to be explored.

As is true of any book, there are passages during which one might stop to quibble with interpretations of previous research. A more sympathetic reading of the new institutionalism literature mitigates much of Grafstein's attack. Shepsle's articles, for example, contrast "structure-induced equilibria" with "equilibrium institutions" in a way that accommodates most of Grafstein's criticism. Shepsle does not treat an institution simply as a "settled pattern of behavior," as Grafstein asserts (p. 85). Institutions are rules and procedures—things that exist apart from convention—that may or may not induce behavior patterns. A configuration of rules lasts (the equilibrium institution continues to exist) until a coalition forms that has the power to change it. This theory does not necessarily carry along with it the functionalist baggage of the older sociological or political conventionalism. One need not suppose that institutions are maintained by a consensus; nor is widespread agreement on the reasons for change necessary for change to occur. Institutions have distributive consequences, and when they are aware of this fact some people strive to change them and others resist.

University of Kansas PAUL EDWARD JOHNSON

The Interpretable Constitution. By William F. Harris II. Baltimore: Johns Hopkins University Press, 1993. 225p. $38.50.

In the best tradition of political philosophy, Will Harris aims to make us puzzle over things taken for granted and to awaken a sense of wonder about the whole enterprise of written constitutionalism. His book often succeeds in provoking the uneasy feeling that time-honored categories of constitutional analysis are radically inadequate. This alone is a profound achievement.

Harris's ambition is no less than the founding of a new paradigm of constitutional studies, one that would leave behind what he regards as the stale dualisms of standard approaches: strict versus loose constructionism, activism versus restraint, substance versus procedure, interpretivism versus noninterpretivism, and positivism versus natural law (pp. 124–31). These unhelpful dichotomies would be case aside to accommodate Harris's complex "theoretics of thirdness" (p. 29).

Harris's third way is not easily described. It is nonfoundational in that it eschews reliance on "external" sources of meaning and authority, including natural law, philosophical theories of justice, and the intentions of the framers where these are not discernible in the Constitution itself. In terms strikingly reminiscent of John Rawls's *Political Liberalism* (1993), Harris argues, "Whatever may be the ultimate 'truth' of the constitutional model from the external perspective, I am not concerned about it. . . . 'Truth' is an inapposite category in political theory" (p. 14).

Harris would have us reconceive the whole genre of constitutionalism as a self-contained practice, "justified not in terms of some higher or firmer independent ground, but owing to its internal sure-fittingness" (p. 11). Written constitutionalism's ambition is, for Harris, comparable to that of Hobbes's sovereign—to ward off chaos by regulating language and reconstituting our lives deeply and pervasively. In the beginning, the sovereign people "speak the Constitution," thereby channeling, bounding, and defining political power and calling themselves into being as a constitutional people. The challenge of the constitutional interpreter is to preserve the existence of this people by moving back and forth between the written document and the constituted political order, interpreting the one in light of the other.

The two orders of *constitutional word* and *policy* (or *Constitution* and *constitution*) are related not simply analogically but "homologically," as mutually constitutive parallel structures (pp. 90–91). The absence of any external grounding "brings on the need to make the internal enterprise very solid" (p. 11). Political wholeness, order, and legitimacy rest on nothing more than "the continual process in which the words of the document and the activity of the polity are aligned" (p. 13). But not any sort of alignment will do. The original sovereignty of the people lies not in their collective willfulness but in the fact that they will *reasonably and as a whole*. Interpreters should reconfirm these elevated qualities by striving to render political power comprehensible and reasonable.

Harris rightly disparages the view (so dear to the law schools) that constitutional interpretation is the special preserve of a professional elite. Authority resides not simply in an original act of ratification embedded in the past but in the continual ratification of conscientious interpreters living under the Constitution's authority. Thus, the Constitution's authority depends on its "legibility" and "interpretability"—meaning that we should strive for a politics in which public officials and citizens take their roles as conscientious interpreters seriously, giving and demanding constitutional reasons in public and insisting on real coherence between interpreted document and political practice.

If Harris owes a debt to Hobbes, he clearly owes one to the ancients, as well: he strives to recover the ancient insight that the political regime pervasively constitutes the character of citizens. Harris's constitution is not merely a form of political power but a "regime," or way of life. The terms and categories of a successful constitution become second nature, providing the political language and self-image of the people as a whole.

Harris casts new light on many constitutional issues. He provides, for example, a useful if sketchy account of popular sovereignty, a conception of which he finds adumbrated in the Constitution's provisions for its own ratification. By identifying a notion of sovereign people implicit in the Constitution, Harris makes it clear that the Constitution conceives of (and indeed derives from) a form of authority higher than those required to amend the Constitution. This has the effect of putting the amending power in its place, contained within, and limited by, the Constitution in various ways. Indeed, Harris interestingly argues that popular sovereignty provides a standard for ranking the authoritativeness of Article V's different modes of proposing and ratifying amendments (by legislature and popular convention).

There is much to admire in *The Interpretable Constitution*, but many will be put off by the book's frustrating elusiveness. Much of this is due, no doubt, to the intrinsic complexity of the argument. Yet Harris asserts at one point that clarity "is a controversial virtue. . . . It has a political program. Its technique is to naturalize its assumptions, to make them seem neutral, transparent, inarguable" (p. 37). Harris means to warn us, I believe, that the demand for clarity may discourage the deployment of difficult abstractions even when they are required by the subject matter. This is a fair point, I suppose, but an author bears the burden of showing that difficult prose is necessary. Some will doubt that Harris has met the burden.

One genuinely interesting feature of this book is that

Harris attempts to combine a moral seriousness akin to Sotirios Barber's (*The Constitution of Judicial Power*, 1993) with a resolute dismissal of moral foundations. The constitutional interpreter seeks, says Harris, reasonableness, coherence, viability, and wholeness but no grounding in external standards or universal moral principles. Harris's Constitution is a constructed and self-contained order—a "public system of signs sheltering a widely available lucidity about public matters," a "stipulated public universe" grounded in nothing but its interpretability (pp. 12, 14, 102). A community of conscientious interpreters, Harris asserts, is self-sustaining and in need of no foundational account of the nature of moral reality.

Harris thus appears to join Rawls in claiming that one can put aside foundational questions without embracing the sort of moral flipness found in much of Richard Rorty's work. Despite their similarities, the postures of Harris and Rawls also seem decidedly at odds. (It is too bad that Harris never engages with Rawls's current project). Rawls would have us put aside foundational issues subject to reasonable disagreement without asserting the truth of a particular reasonable view (the range of reasonable foundational views being limited). Rawls does not deny that there is a "true" account of the trust as a whole, he only asserts that reasonable people can disagree (within a certain range) about what the truth might be.

Harris's constitutionalism, on the other hand, sometimes appears to rest on a very definite and particular foundational view—a morally skeptical conventionalism. Harris relies, after all, upon a distinctively Hobbesian rendering of the problem of political order. Hobbes emphasized the radical nature of prepolitical disorder in order to justify political absolutism. Harris also views constitutions as hedges against primal chaos and "the nothingness of disorder and darkness" (p. 49; see also pp. 51, 73, 107, 174, 176). Harris is not out to justify absolutism; rather, he deploys the trope of the darkness and chaos outside the constitution's bounds to emphasize the absence of any external grounding for constitutional meaning. All this also serves to dramatize the Constitution's radically constitutive consequences for social life and citizen character.

It is odd, however, to portray the Constitution as independent of a variety of preexisting sources of order and meaning: the British constitutional tradition, shared beliefs about natural rights, the family, economic markets, the state constitutions, and the English language. Even the Declaration of Independence is completely missing from Harris's account. Harris's point seems to be that none of these sources provides an external standard for settling constitutional meaning, any more than do natural law or the unwritten intentions of the framers. To posit such an external source of meaning would make a mockery of the form of words that were solemnly ratified, that claim to be supreme law, and that we take (or should take) so much pains to interpret; it would deny the fundamentality of this form of words. Talk of darkness and chaos beyond the constitution's well-lit space may not, then, be intended to disparage the importance of history, rights theory, and other matters. It may only imply that all claims to constitutional meaning must be cashed out from within the (at least somewhat) autonomous enterprise of constitutional interpretation.

Harris is acutely aware that the success of the Constitution depends upon its ability to fundamentally reconstitute the polity as a whole in its own image. The radicalism of Harris's constitutionalism is a useful corrective to the bland superficiality of the dominant legalistic models and some liberal theory. Even so, it is far from clear that the need for such a corrective justifies Harris's claim (recurring and melodramatic) that only void and darkness lie beyond the Constitution's orderly and well-lit space. The Constitution constitutes radically—transforming families, churches, and even language in its own image—but it does not create order out of sheer chaos. The Constitution constitutes, but only to a degree. If Harris's view were to take hold, his own corrective would need correcting.

The Constitution is one of our shared practices, but not the only one. Philosophy, history, comparative politics, and other disciplines allow us to imagine alternative constitutions and to keep some critical distance on our own. The very radicalism of Harris's constitutionalism puts that distance in jeopardy. That radicalism and its implicit flirtation with nihilism also jeopardize the faith in public reason that elevates Harris's conception of political authority above mere willfulness.

Perhaps, however, Harris's point is that within a successful constitutional order such as ours, real critical distance is far more difficult to achieve than is usually imagined. The Constitution constructs its ideal readers as its citizens. One might always suspect, therefore, that claims to critical distance are a conceit—that the only standpoints available are hopelessly in thrall to the deep practices and presuppositions of the regime that simply make us what we are. Such reflections can indeed lead one to entertain nihilistic possibilities, and here, again, Harris has a disquieting point.

Harvard University STEPHEN MACEDO

Algernon Sidney and the Republican Heritage in England and America. By Alan Craig Houston. Princeton: Princeton University Press, 1991. 335p. $39.50.

Algernon Sidney was executed in 1683. He was suspected of plotting to assassinate Charles II, king of England, and his brother James, Duke of York, the future James II. In the absence of the two witnesses required by the English law of treason, the court allowed an unpublished theoretical treatise, the *Discourses Concerning Government*, to be admitted as evidence against him. When the *Discourses* were published in 1698, edited by the pantheist and freethinker John Toland, they established Sidney's enduring reputation as a martyr for liberty. Throughout the eighteenth century Americans referred to Sidney and Locke as the two great theorists of political rights, and it is possible that Sidney was more influential than Locke.

There are two reasons now for turning to Sidney's *Discourses*. First, Sidney was one of three authors to write a book-length refutation of James Filmer's defense of absolute monarchy, *Patriarcha*, shortly after it was published in 1680. The other two were James Tyrrell and John Locke, and the similarities, differences, and possible relations of influence between the three works are matters of continuing debate.

Second, Sidney (who fought for Parliament in the Civil War and spent years in exile after the Restoration) was widely regarded as an obdurate republican. Ever since the publication of John Pocock's *Machiavellian Moment* (1975) scholars have been studying the links between seventeenth-century English republicanism and the American Revolution, and debating the claim made by Pocock, Gordon S. Wood, and others, that John Locke, liberalism, and the natural law tradition of argument were relatively insignificant in the making of a revolutionary consciousness, while a contrasting republican tradition, concerned with virtue, corruption, and citizenship, rather than rights and obligations, was all-important.

Modern Sidney scholarship may be said to begin in 1978, when Blair Worden announced the discovery of the *Court Maxims*, a previously unknown (and still unpublished) political treatise by Sidney, apparently written in 1666. Since then, four volumes, apart from Houston's, have been devoted to Sidney: Paulette Carrive (1989); John Carswell (1989); and Jonathan Scott (1988 and 1991). Of these authors Houston is undoubtedly the best. He is learned and accurate; moreover he writes well, and argues clearly. (Some of the best arguments and liveliest quotations are concealed, in the manner of Gibbon, in the footnotes.) As a thesis this study received the Leo Strauss Award, and the book has gone on to win the Foundations of Political Theory Book Award.

Houston begins by providing an admirable summary of what is known of Sidney's life and political activities, and a sketch of the royalist theories he opposed. It is particularly helpful to have an explanation of why some scholars have argued that Filmer's views were typical of Tory thinking, while others have insisted they were exceptional. At the heart of the book stand three chapters on the political theory of the *Discourses*. These stress that Sidney believed in popular sovereignty and the separation of powers, not in "mixed government" as that term is usually understood. They are followed by an important final chapter on Sidney's influence in America.

The book's central argument is a simple one: Sidney writes in the natural law tradition of Grotius as much as he does in any republican tradition. Eighteenth-century readers usually coupled Locke and Sidney together, and felt under no obligation to choose between them. Scholars who follow Pocock and Wood in contrasting republicanism to natural rights theory obscure the presumption of contemporaries that the two complemented each other. (A similar argument has been made with regard to *Cato's Letters* by Ronald Hamowy).

On this question I find Houston entirely convincing. I am not persuaded, though, by his further claim that Machiavelli was not an important influence on Sidney (pp. 164–5, and other places listed in the admirable index.) I find it hard to read Sidney on the need to foster a warlike people (2.15, 2.21, 2.22, 2.23: "That is the best government which best prepares for war") or on civil disorders (2.24) without sensing the influence of Machiavelli. When, for example, Sidney defends "extrajudicial proceedings" and "extraordinary ways" I suspect he is translating Machiavelli's *modi straordinari*. It is true that there are numerous Machiavellian themes which do not interest Sidney (p. 165), but it is not true that for Machiavelli corruption and change are synonymous (p. 218): when Sidney criticizes those who think they are it is presumably Harrington, not Machiavelli, he has in mind.

If Houston underestimates Machiavelli's influence, he is right to insist that republicanism and Machiavellism should not be treated as synonymous, and that Machiavellism should be regarded as a range of preoccupations, not a single theory. He correctly notes that Sidney's favorite author is not Machiavelli but Tacitus (p. 154). Of course seventeenth-century readers often turned to Tacitus to find there the arguments of Machiavelli, an approach which had been particularly encouraged by Traiano Boccalini, whom Sidney twice cites with approval (2.21, 2.30). But, like Sidney, Boccalini was interested only in a few specific aspects of Machiavelli's thought. It would have been interesting to compare Sidney with Boccalini, for both played a crucial role in adapting the republican thought of the Renaissance to make it serviceable in nation rather than city states. Such a comparison might have provided the basis for an alternative account of the republican tradition to that proposed by Pocock.

It is a sign of a good book that one wishes the author had written more not less. Houston describes the argument of Sidney's *Discourses* as "a sunken ship: though a few promontories are visible above the surface, the great hulk remains submerged in the depths" (p. 180). He has done an admirable job of salvaging what can be recovered from the wreck, and is persuasive in his claim that its design was less antique than has previously been believed. Sidney died so that his beliefs might live: thanks to Houston we now have a better understanding of what those beliefs were.

University of Victoria DAVID WOOTON

Justice between Age Groups and Generations. Edited by Peter Laslett and James S. Fishkin. New Haven: Yale University Press, 1992. 243p. $30.00.

Choice over Time. Edited by George Lowenstein and Jon Elster. New York: Russell Sage Foundation, 1992. 352p. $39.95.

The issues of maintaining the quality of the ecosphere, the sustaining of the modern welfare state, and the restructuring of our national debt are each of immediate interest and of concern for those who examine intergenerational equity issues. More generally, those who look at intergenerational issues must concern themselves with how humans make decisions regarding outcomes that occur in future time periods. Similarly, those who must analyze how we choose from among alternatives with payoffs in the future will want to consider applications that go beyond one person's lifetime (i.e., intergenerational problems).

The Laslett and Fishkin volume is a collection of 11 essays that can serve as a serious introduction to some of the philosophical issues regarding intergenerational justice. The Lowenstein and Elster volume explores some of the experimental and other empirical findings and technical theoretical issues involved in explaining choice behavior involving delays and time. Although both books are of considerable depth and quality, neither really integrates the two concerns of intergenerational choices and choice with a time dimension. Thus, the student of either subject may wish to look at the two together for a rounded grasp of the subject. The pair of essays that Tyler Cowen helped author in the Laslett and Fishkin volume, analyzing the issues surrounding the discounting of future payoffs in choice, comes closest to dealing with both issues at the same time.

But the lack of integration of the subjects of the two should not be taken as a harsh criticism of either. The Laslett and Fishkin volume contains a number of very interesting essays for those of us concerned with public policy or intergenerational equity. Although many may be aware of the longer history of welfare policies in the West, the historically oriented essay by David Thomson, "Generations, Justice, and the Future of Collective Action" is still worthwhile to jog our memories. Thomson summarizes some of his other research to show that the current insurance type of welfare state is historically not likely to be considered stable. Large transfer programs of the sort currently offered in our modern welfare state also were around in the nineteenth century and earlier. They all went through cycles of lack of popular support. Thomson then argues that such instability threatens the insurance value of the state programs and hence their value in the eyes of the citizens. He also makes the somewhat more controversial and original argument that these social (or entitlement) programs really are a redistribution to a particular generation. All in all, this is a controversial and enlightening piece.

Most of the other papers are substantially more philosophical in nature, and some of these are very strong indeed. Especially helpful were the pieces by George Sher ("Ancient Wrongs and Modern Rights") and James Fishkin ("The Limits of Intergenerational Justice"). Sher's piece is a very useful extension of Nozick's argument regarding compensation over time. Sher investigates the conditions under which the justification for compensation decreases as the event is further in the past, and he does this in a highly sophisticated and interesting fashion, dealing with such issues as the cross-generational consequences of behavior.

Fishkin, on the other hand, indicates how we might have to consider new foundational concerns if we wish to extend ethics across intergenerational issues. For example, traditional theories of harm and torts maintain individual identities as specific arguments in the welfare arguments (e.g., if you do X, it would hurt that particular person, i, and thus you should offer to i compensation). Fishkin shows that these sorts of argument are problematic for many (if not most) intergenerational equity issues. Juxtaposing this class of theories with "individual-identity-independent" theories of ethics, he shows that the theorist has no strong options. Fishkin then sketches how some of his earlier published ideas can be extended to perhaps give us some leverage in these situations.

Some of the other pieces (e.g. those by Braybrooke and Epstein, and the introduction were a bit less interesting. Braybrooke and Epstein restate quite classical and opposing ideological positions that are not really worth reviewing. And Laslett and Fishkin's introduction was less a guide to the skeletal structure of the volume than it might have been.

As indicated, the two pieces by Cowen can be seen to serve as a bridge between the two books, especially "Consequentialism Implies a Zero Rate of Intergenerational Discount." This essay is in the style of social choice proofs and includes a demonstration that (1) Pareto indifference, (2) transitivity of indifference, (3) neutrality (between persons within generations), and (4) well-defined preferences across living in different eras imply that one have no discounting of future states. Although there were some problems with the analysis, the argument was valid and interesting. Validity, of course, does not imply soundness; and among my objections to the soundness of his claims, I would include his assertion that the four conditions are equivalent to consequentialism, that is, are needed to evaluate the qualities of an outcome (e.g., in cost–benefit terms). Also, Cowen does not properly deal with some of the difficulties in the four conditions. For example, the axiom of Pareto indifference has numerous limitations, which are not properly fleshed out. Indeed, its problems are similar here to its problems in the case of noncomparative utilities in Sen's proof that we cannot make judgements about redistribution. But the chapter is still a very interesting point of departure.

Choice over Time is concerned with *individual* choice when payoffs associated with alternatives are best understood as varying in their time of receipt. The concerns addressed here are primarily those of the modeler and theoretician in the psychology, economics, and public choice traditions. Within this book, one finds considerable information about the problems, findings, and theories of individual choice with delayed payoffs. The difficulties and models outlined by the authors here are quite analogous to the conundrums and anomalies developed by Kahneman, Tversky, Grether, and others in the areas of choice under risk. The researchers interested in prospect theory and other such models will find this volume quite enlightening. The very useful introductory essay weaves a demanding collection of essays into a manageable intellectual tapestry. The utility of the introduction is greatly enhanced by a second essay by Lowenstein, giving a sketch of the intellectual history of this field. But getting into the remainder of the volume in depth is a daunting affair, unless one has previously invested heavily in this area. In too many of the essays, the technical terms are not defined, and the models are not carefully explicated for the new comer. This failed task leaves the volume of serious interest almost exclusively to those who are already literate in research in the area of time-discounted choice. The essays contain reports and critiques of virtually all the important experiments. The major controversies and models are aired and even, in some places, extended.

The foundational conceptualization of choice concerning delays serves as a solid foundation for a number of alternative conceptualizations of the problem, including concerns with the shape and contents of the discounts. Essays by Mischel, Shoda, and Rodriguez; Ainslie and Haslam; Rachlin and Raineri; and Lowenstein and Prelec investigate these alternative foundations. They further lead to a common theme, or unity of perspective, that persuasively paints the area's major anomalies (e.g., predictable patterns of preference reversals over time as generalizable models of discounted choice over time. Indeed, this is done with considerable panache and intellectual sweep. Ainslie and Haslam, for example, move from the problems of general personality instability (e.g., as manifested in extreme cases via multiple personalities) to more ubiquitous preference instabilities.

Even though some of the findings are often found more compellingly developed elsewhere, this collection shines as a serious dialogue over the issues and extensions that are at its heart. For example, the essay by Schelling on self-control is an echo of his pieces in the *American Economics Review* and *Public Interest*. But here we find Shelling's argument "formalized" and placed in a tradition of hyperbolic discount functions in a second essay by Ainslie and Haslam. This generalization enables us to see the general problem of addiction and disfunctionality (as well as self-control) as special problems in the area of discounted choice. As a general problem, self-control is further developed in three other essays by Shefrin and Thaler; Becker, Grossman, and Murphy; and Herrnstein and Prelec. With such juxtapositions and selections, Elster and Lowenstein have put together quite an intriguing volume.

In sum, those nonneophytes interested in micro issues of choice involving payoffs through time will find the book of value. And those interested in the macro policy concerns of the subject (e.g., as dealt with in the Laslett and Fishkin volume) should find the theoretical and empirical contents of the book of considerable use in their positive analysis of the choices over the policy alternatives.

University of Maryland JOE OPPENHEIMER

The Social Cage: Human Nature and the Evolution of Society. By Alexandra Maryanski and Jonathan H. Turner. Stanford: Stanford University Press, 1992. 213p. $39.50 cloth, $12.95 paper.

Unlike the vast majority of theory works in social science, Maryanski and Turner embrace natural science as a relevant source of theory, data, and methods that social scientists can employ the better to understand human social behavior. In particular, this book deems evolutionary theory and primatology to be indispensable to realistic analysis of the social biological adaptation of contemporary human societies. *The Social Cage* is authored by sociologist biosocial-theorists. It was evidently written for sociologists, but it is certainly readable by political scientists. Members of the APSA's politics and life sciences section agree with Maryanski and Turner about extending historical time by using evolutionary theory in a serious way; insisting upon conceptualizing humans and primates, whose big brains are responsible for their unique cultural constructions; and insisting also upon transcending the usual social science cultural determinism by substituting a transactional biosocial approach to any study of human social organization, communication, and behavior generally. There are, of course, hundreds of such works by physical anthropologists, as exemplified by Adrienne Zihlman's *The Human Evolution Coloring Book* (1982).

Maryanski and Turner's discussion of human animality is exceedingly brief and relies much more than necessary on pop ethology, instead of the standard works by zoologists who teach and do research in the field of animal behavior. The authors' selection of sources for, and coverage of, primate systematics is much better done, both qualitatively and quantitatively. However, they do represent the organizational (and even more important, the aggressive) behavior of common chimpanzees by giving major credence to the later, rather than the earlier, work of Jane Goodall. Of course, the earliest work from 1960 to 1963 was done by Jane herself, observing the Gombe chimpanzees in a relatively much more natural setting than later with dozens of collaborators, after the human political, economic, and social environment caged her research enclave in. Anthropologist Margaret Power has pointed out that Maryanski and Turner "do rely wholly on reported behaviour of the Gome (and Mahali) studies of abnormally stressed chimpanzees, in drawing up Table 1. This results in a picture of the natural behaviour of chimps generally which is the opposite to what they would have found, had they drawn on the naturalistic studies of undisturbed (not artificially fed) apes. The result is that they propose an extremely misleading understanding of the probable behavior and relationships of our hominid ancestors" (personal communication). Moreover, as nonexperts in the subject they summarize, they give the impression of being perhaps overly impressed with such methodological innovations as cladistics and its imputed power to determine what pre–World War II paleoanthropologists called the "Missing link," which Maryanski and Turner denote as the *last common ancestor* among humans and other great apes. On the other hand, their detailed cross-species analysis of the species-specific social network characteristics among both hominoids and Old World monkeys, proffers a provocative approach for generalizing about the social organization differences among and between apes and monkeys.

They take quite a bullish stance on the question of the language/mental abilities of chimpanzees and gorillas, which they manage to do at least in part by overlooking such skeptical works as Thomas Sebeok and Jean Umiker-Sebeok's *Speaking of Apes* (1980). Much better is their discussion of psychobiology in relation to the transactional development of brain size and language among hominids over the last four million years and especially the last million years, together with a good summary of the complex relationships between primate/human perceptive sensory organs and cognitive phylogenetic development. Then they relate sensation-cognition-language to hominid/human behavior—in response to ecological changes through evolutionary time—as determinants of the social communication basis for hominid social organization and behavior. That, in turn, resulted in the extraordinarily stable forager, gatherer, and hunter niche that characterized *Homo erectus, Archaic homo sapiens, Homo sapiens Neanderthalis,* and *Homo sapiens sapiens* for 1.5 *million* years, in contrast to the maladaptations of the past 12 thousand years (i.e., less than 1% of the gathering–hunting base time).

Maryanski and Turner assert that when the hunting–gathering niche was universal (initially, in Africa then later through the Middle East to Europe in the West and Asia in the East), humans all lived in a correspondingly universal Garden of Eden—as compared to the niches that followed during and after the transitions to herding, gardening, agriculture, industrialism, and postindustrialism. In this regard, it is evident Maryanski and Turner, in their discussion of preindustrial societies (chaps 4–5) follow closely the theory and terminology advanced in the sixth edition of a more than 20-year-old leading text in biosocial sociology, which anticipates the same argument about the relative similarity of industrial societies to gathering and hunting groups, in regard to the putative isomorphism between the evolved species genome and the benefits of the adaptation in terms of such positive values as egalitarianism, leisure, freedom, and so on (Lenski, Lenski, and Nolan's *Human Societies* [1991], 403–37). Also, includes in his earlier introductory text in general sociology a briefer but theoretically identical statement of the present discussion of preindustrial and industrial societies, except for a less extensive development of social organization network theory (*Sociology*, 1985. chap. 4).

Gathering and hunting was an Eden because population sizes and levels remained sufficiently low that adequate food (both plant and animal) could be acquired with relatively little time and effort by small groups of humans who moved about opportunistically, continuously resettling their habitat. No one could amass material wealth; leadership was multiple-role-oriented; males and females were deemed to be equally important; and the group was also egalitarian in its economics, society, and politics. This, in turn, promoted a high degree of individualism. Over the period of 1.5 million years, the human species genome was shaped by selection for phenotypes that could behave in ways appropriate to the gathering-and-hunting niche; and that adaptation become genetically fixed by perhaps a hundred thousand years ago, by which time both brain size (weight) and language capacity had stabilized. (This interpretation of human phylogenetic development is quite conventional among paleoanthropologists and paleo-human biologists. It may be novelty for most political scientists but not, of course, sociologists.)

The *social cage* consists of the increasing constraints on human individual and social freedom imposed by the categorical imperatives of humans' adapting to "civilized" life as the caretakers of domesticated plants and animals (as Maryanski and Turner explicate in great detail in their chapters on horticultural and agrarian societies). Indeed, by the time one reaches their concluding chapter, *cage* becomes a much-overworked metaphor for human *self*-domestication.

An especially novel—and to me perplexing—section occupies the latter part of chapter 7 (on industrialized societies) and chapter 8. Thus, "industrial societies . . . [are] far better than their predecessors because of their capacity to free people from much physical drudgery and to give them wide arrays of options and opportunities and much more say in how they run their lives" (p. 140); yet they also assert that "kinship in industrial and post-industrial systems completes its odyssey back to its original nuclear profile in hunting and gathering societies" (p. 157). They had previously characterized kinship in hunting and gathering societies as involving "neolocal, bilocal, and patrilocal" residence and a "clear division of labor; males hunt, and females gather and do domestic chores" (p. 86) which seems difficult to reconcile with contemporary yuppie family life in postindustrial societies. Margaret Power has emphasized that the impact of kinship throughout human evolution has been to establish *bonding*, not caging; and she shows that the patrilocal band model, as distinguished from the forager model that characterized human organization throughout the initial 99% of human organization throughout the initial 99% of human existence, reflects *pre*-1960s anthropology (*The Egalitarians—Human and Chimpanzee*, 1991).

Maryanski and Turner further state that "the state is transformed with industrialization, becoming much more democratic and larger" (p. 157). Again, "The changes associated with post-industrial systems are more compatible with humans' biological nature than those occurring in earlier ones" (p. 161). Again "[Postindustrialism is] a system comparatively well attuned to [humans'] ape ancestry . . . [and] in truth ordinary people embrace the chance to live and participate in a system relatively well attuned to their primate heritage" (p. 162). In other words, industrial society, technology, postindustrialism, and our contemporary nuclear-computer economy and society have "liberated" humans from their agrarian bondage, so that individualism is on the rise again; and, through the blessings of industrialism and technology (as exemplified by electronic kitchens that free their possessors to see professional employment meet to their high education and skills), they are living more like their gatherer-and-hunter forebears than any of their immediate foremothers-and-fathers could do during the 10 thousand years or so preceding the nineteenth century in England, Europe, and North America.

I have no doubts that industry and technology have given Jane Fonda and Ted Turner a relatively greater freedom that the average gardener or farmer—or even gatherer—hunters might once have enjoyed. Maryanski and Turner might be more persuasive if they attempted to demonstrate *empirically* that most people in modern industrial/postindustrial societies are upper-middle-class or better and are politically equal in fact and that they enjoy the leisure of a four-hour (i.e., a gatherer–hunter) working day. But the facts are that yuppies and libertarians and Perotistas are a small minority even in such great capitalistic societies as the United States and Japan and few of them are on four-hour workdays. I do assert—and on empirical grounds—that the social cage of postmodernist postindustrial technological life is the opposite, in its effects, to what Maryanski and Turner claim and hence that their concluding two chapters have got it backward. The social cage today is exponentially worse than that of agrarianism with regard to the economic security and the political and social freedoms that for most people make their lives worth living.

Nevertheless, and irrespective of my caveat on the empirical question whether contemporary humans are now occupying a niche that resembles that of gathering-and-hunting peoples, this book will be informative and enlightening to the many political scientists who attempt to teach and research politics while at the same time ignoring evolutionary theory, human evolution, and the biosocial aspects of human social organization and political behavior. I recommend *The Social Cage* to political scientists as a novel, intriguing, and exciting approach to understanding the relevance and importance of a great deal of "non–social science," that is, biosocial research findings relating to human communication, social organization, and political behavior today. Political behavior has been a reality since *Homo sapiens* began some 50 thousand years ago and, to some extent, even before then in the social life of our hominid and, earlier, hominoid forebears.

University of Hawaii, Manoa GLENDON SCHUBERT

Foundations of Liberalism. By Margaret Moore. New York: Oxford University Press, 1993. 222p. $45.00.

This is a very good book. Margaret Moore critically discusses, with admirable clarity and fairness, the work of a number of leading contemporary liberal thinkers, including Gewirth, Rawls, Gauthier, Larmore, Kymlicka, and Raz. Informed by a sympathetic appreciation of contemporary communitarian thought, her judgments regarding liberal theory, though often critical are not marred by the strident, polemical tone too often found in similar exercises. The analysis is detailed and careful, with far more insights generated than axes ground. Along with Will Kymlicka's *Contemporary Political Philosophy* (1990), this book provides one with a sound and sophisticated overview of the "main positions" staked out in contemporary liberal theory. But it will also be of interest and benefit to those already familiar with these theorists and their critics. I have read a great deal of material dealing with liberalism and communitarianism over the past five years, and this ranks with the best.

The analysis is organized around the contrast between the personal point of view and that of the impersonal or impartial perspective of morality. The various liberal theories are evaluated in terms of the facility (or lack thereof) with which they articulate a compelling solution to the "integrity problem" (explaining how the personal and impartial perspectives are related) and the "motivation problem" (explaining why the person has reasons to act in accordance with the demands of impartial morality). In part 1, Moore applies this analytical framework to the theories of Gewirth, Gauthier, and the Rawls of *A Theory of Justice*, treating each as an attempt to resolve these problems. While seeing somewhat less merit in Gauthier's version of instrumental morality than Moore does, I nevertheless found her treatment of all three to be enlightening. My guess is that of the thinkers treated in the book, Gewirth's views are the least well known to political scientists; Moore's chapter on him will serve as an excellent introduction and critical overview. The chapter on Rawls is also quite good. I was especially intrigued by Moore's comments on the relationship between the structure of the family as an institution and the development of moral sentiments; they make for an interesting comparison with those of Susan Okin expressed in *Justice, Gender, and the Family* (1989).

In part 2, Moore considers contextual (Larmore, the later Rawls) and perfectionist (Raz, Kymlicka) arguments for liberalism. Moore's evaluative stance toward these views is somewhat more equivocal than the straightforwardly negative conclusions advanced in regard to the theories treated in part 1. This is because the views treated here (especially those of the later Rawls and Raz) are in important respects, similar to the views Moore herself advances in part 3 as an "alternative foundation for political and ethical principles." Moore acknowledges these similarities openly, while nevertheless rejecting the theories of (the later) Rawls and Raz in their present formulations. This difficult balancing act is well executed, and the resulting discussion of possible strategies of articulating and defending liberal principles (or political principles generally) will be thought-provoking and illuminating to anyone concerned with such matters.

The similarities are, roughly, as follows. Moore's alternative foundational views include (1) a conception of the *person* that accepts the notion of a constitutive tie between personal identity and the social relations characteristic of a particular community; (2) a conception of *justification* (of ethical and political principles) sufficiently modes, local, and historically bounded not to aspire to universal or timeless validity; and (3) an affirmation of the perfectionist view that the state can be "justified in acting to protect and promote valuable ways of life. State action in support of certain ways of life may be necessary to ensure a flourishing and valuable culture, in which meaningful choice is possible" (p. 193). Rawls now seems to endorse view 2 and arguably view 1, while Raz endorses views 1 and 3 and arguably view 2. Moore nevertheless criticizes Rawls and Raz on the grounds that the liberal *political* principles each endorses are ultimately inconsistent with these foundational *philosophical* claims.

There is, in my view, a good deal to be said for versions of all three views and hence for the foundational views that Moore skillfully defends in part 3. I was not as convinced by her arguments in support of the claim that liberal political principles are, of necessity, in a contradictory relation to these foundational views—which is another way of saying that I think Rawls' and Raz's liberalisms more defensible than does Moore. Readers will have to judge this and much else for themselves, of course, but they may be assured that reading Moore's book will provide much stimulation toward doing so and a fine example of how to do so thoughtfully.

University of Vermont PATRICK NEAL

On the Edge of Anarchy: Locke, Consent, and the Limits of Society. By A. John Simmons. Princeton: Princeton University Press, 1993. 293p. $39.50.

In this work, the author seeks "to present, analyze, and to a certain extent defend . . . John Locke's own theory in his *Two Treatises of Government*." In addition, *On the Edge of Anarchy* tries to present "the best possible defense" of Locke's political philosophy, although this may involve "excising or dramatically revising" Locke's thought, as well as subjecting it to "a contemporary critique" (pp. 3, 7). What we are promised, then, is that "the new Lockean view that will emerge at the end of this work is a better, clearer, more helpful view than Locke's own" (p. 9). This approach was adopted by Simmons in *The Lockean Theory of Rights*, and, as a comparative assessment, it has to be said that the objectives of Simmons's methodology were much more successfully realized in the earlier work. Whereas it was concerned only with Locke's theory of rights and not his whole political philosophy, the present book reverses this emphasis. One of its defects lies in Simmons's truncated version of what constitutes the "whole" of Locke's political theory.

On the Edge of Anarchy is structured around a discussion of Locke's account of the state of nature, his theory of consent, and his theory of resistance—important elements of Locke's political thought, to be sure. While these elements are considered in relation to each other, much of what gives meaning to "Locke's own theory" is omitted from the discussion altogether. This is because whereas Locke's meaning is often embedded in the political conflicts of seventeenth-century England, Simmons's meaning is often derived from contemporary philosophical definitions.

For example, in discussing Locke's definition of the state of nature as a condition lacking "a common judge with authority," Simmons argues that Locke cannot really mean that want of a common judge is a definitional feature of the state of nature. This argument is premised upon a rather sharp distinction between a "judge" and a lawmaking body, so that (Simmons concludes) a "'common judge' would appear to lack the right to make law for those he judges between" (pp. 18–19). But this enforces a modern distinction that Locke and his readers would not have felt compelled to make, since they regarded Parliament as both a lawmaking and a judicial body. So when Locke writes that what "puts men out of a state of nature into that of a commonwealth, by setting up a judge on Earth with authority to determine all the controversies, and redress the injuries, that may happen to any member of the commonwealth; which judge is the Legislative, or magistrates appointed by it," it is clear that for him, the "judge" and the lawmaking body are the same (*Second Treatise* 89). Moreover, this perspective is part of another argument that Locke is making here, namely, that no one could "think themselves in civil society, till the Legislature was placed in collective bodies of men, call them Senate, Parliament, or what you please"; and this civil society, with the legislature as the common judge with authority, is again contrasted with the state of nature by mutually exclusive definitions (ibid. 94). What is obscured in Simmons's discussion is the political importance of this argument against absolute monarchy, which (as Locke defines it) is simply the combining of executive and legislative power in the hands of a single individual (ibid. 91). Locke's conception of a limited monarchy in terms of *institutional* limitations (i.e., a division of executive and legislative power) is thus important for his defense of "civil society" as a particular kind of institutional arrangement or distribution of political power, contrary to Simmons's reading of the text, which seems to imagine that Locke has an interest in defending a notion of "limited monarchy" as a legitimate form of government apart from the institutional division of powers just described above (ibid.).

In discussing natural rights, Simmons argues that there is "a distinct lack of evidence" that Locke "took himself to be defending a thesis of inalienable rights" (p. 119). Much of this discussion is concerned with arguing that since we do not possess certain rights (e.g., the right to consent to kill ourselves), we cannot alienate what we do not have (pp. 120–21). Armed with this philosophical pronouncement, we may then dismiss virtually all of the Lockean candidates for inalienable rights, including the right to religious toleration (pp. 108–46). This seems an example of the kind of word manipulation that frequently passes for philosophizing. If I am entrusted (by God and natural law) with a right to choose my religious beliefs and, Locke argues, this right can neither be delegated to another nor taken away from me by another, this right to choose would normally be understood by individuals living in the seventeenth century as an inalienable right. Moreover, as part of a political debate, the claim for an inalienable right plays an important role in undermining the claims for authority advanced by Locke's opponents.

Simmons maintains that while Locke defends a right to resistance to tyranny, he does not argue that we have a duty to resist. According to Simmons, Locke believes that "we have the right to aid the oppressed, but not a duty to do so," although Simmons believes that Locke *should* have defended the duty of the people to resist injustice (pp. 179–81). Again, this seems to me to be a play on words at Locke's expense. For, in the seventh of the *Essays on the Law of Nature*, Locke elaborates upon the duties and obligations placed upon individuals by natural law, which include "the relief of one in trouble," as well as "the consoling of a distressed neighbor" and "the feeding of the hungry." It seems to me that "the relief of one in trouble" covers the case of individuals being wrongly imprisoned and/or tortured because of their religious or political beliefs, which was certainly relevant to the argument for the right to resistance Locke was making in the *Two Treatises*. The fact that these duties (including charity to the poor) depend upon individuals' having the resources to implement them (which, in the case of resistance, may, as a prudential matter, require at least the tacit support of the majority) does not, in Locke's view, remove them from the status of being duties, as Simmons suggests.

The major portion of *On the Edge of Anarchy* is concerned with Locke's theory of consent and various suggestions for improving that theory. "Locke's real mistake," Simmons argues, "was in saying that one could only become a member [of political society] by express consent" (pp. 89, 194). Since Locke means actual personal consent (p. 60), we are faced with either an empirically implausible condition for establishing the legitimacy of government or with the attempts of Locke (and other consent theorists) to conflate express and tacit consent to the point where the very meaning of express consent is undermined (pp. 86–87, 197–269). Moreover, Simmons follows John Plamenatz and others in arguing that Locke's usage of the term *consent* has no reference to any particular form of government (p. 91). Hence, as a general characterization, "the problem for consent theory . . . [is] one of *realism*" (p. 199). It is hardly possible here to discuss (let alone resolve) the various problems arising from consent theory; but instead of condemning poor Locke for his lack of realism, it might be more profitable to recognize the existence of at least three linguistic conventions in seventeenth-century England that did associate consent with the existence of elections and democratic practices, so that the *historical* plausibility that Locke's discussion of consent in the *Two Treatises* is grounded in a realistic attempt to associate political legitimacy with the existence of such practices can at least gain a fair interpretive hearing, instead of being dismissed out of hand as an expression of Locke's "own meaning" (as it generally is by philosophically oriented interpreters of that work).

None of my comments are meant to suggest that *On the Edge of Anarchy* is a bad book. On the contrary, Simmons has many intelligent and thought-provoking things to say about Locke, and he is both painstaking and comprehensive in his efforts to relate his own position to the existing secondary literature on Locke. Even so, *On the Edge of Anarchy* does demonstrate that there is still a gap between those who believe, with Plamenatz, that history and politics simply provide the background information for philosophical analysis and those of us who argue that what the philosophical problems *are*, for a thinker like Locke, cannot be stated independently of a contextual (political/historical) knowledge relating to the formulation of these problems.

University of California, Los Angeles RICHARD ASHCRAFT

Liberal Nationalism. By Yael Tamir Princeton: Princeton University Press, 1993. 194p. $24.95.

Yael Tamir's *Liberal Nationalism* is a philosophical essay on a subject of central importance in today's politics. Inspired by her involvement in the Israeli peace and civil rights movements, it attempts to reconcile two sets of seemingly contradictory values on which both movements are based: the values of liberalism and of nationalism. The purpose of the book is to redefine "the national vision" in the light of liberal values, to place "national thinking within the boundaries of liberalism" (pp. 3–4, 12). Tamir believes in the possibility of "an ideal marriage" between the two, in that "the liberal tradition with its respect for personal autonomy, reflection, and choice, and the national tradition, with its emphasis on belonging, loyalty, and solidarity, although generally seen as mutually exclusive, can indeed accommodate each other" (pp. 6, 140). Such accommodation, she claims, will benefit both traditions: liberalism will be rendered more attuned to people's deep affective needs, as well as more honest (aware of its own hidden nationalist agenda) and less egoistic, while nationalism will thereby be enabled to meet "the challenges of the next century" (p. 77).

Liberalism, in Tamir's discussion, is defined essentially as "a theory about the eminence of individual liberties and personal autonomy"; nationalism is defined as a theory "about the eminence of national–cultural membership and historical continuity, and the importance of perceiving one's present life and one's future development as an experience shared with others" (p. 79). Speaking about nationalism, Tamir employs the terms of communitarian discourse, which, she claims, is "akin to the nationalist one in its content" (p. 14). The "nation," on which the definition of nationalism is predicated, is seen as a form of cultural community "imagined" by its members as distinct from others. Tamir is aware of the "illusive" quality of this definition, which does not allow to distinguish between nations and other cultural communities, but she believes that a more satisfactory definition is impossible (pp. 9, 66). The model of liberal nationalism that Tamir offers presupposes the "contextual individual," for whom personal autonomy is as essential as embeddedness—the two needs liberal nationalism satisfies (pp. 33, 104–7). Liberal nationalism is polycentric, it "is predicated on the idea that all nations should enjoy equal rights, and in fact derives its universal structure from the theory of individual rights found at its core. If national rights rest on the value that individuals attach to their membership in a nation, then all nations are entitled to equal respect" (p. 81; see also p. 9). Liberal nationalism is best realized in the framework not of nation–states but of multicultural regional entities—political, military, and economic unions of separate but equal nations—each assured the conditions for cultural self-determination, which enable individuals to choose their national identities for the only relevant (i.e., "national–cultural") reasons (pp. 151–57, 163).

Tamir is aware that "recent versions of nationalism seem to lend little credence to the liberal nationalist position" she offers. In fact, she says, "a cursory glance at the surrounding reality could easily lead to the conclusion that liberal nationalism is a rather esoteric approach" (p. 117). Why should this be so? Why should nationalism, in reality, so often take illiberal forms and inspire political systems not even remotely resembling Tamir's ideal vision? The book does not address these questions and, taken on its own terms, cannot be criticized for that. It is an essay in philosophy, a "normative [and] programmatic" statement, rather than empirical investigation; Tamir's goal is to suggest a model of nationalism compatible with liberal tradition, not to explain nationalisms as they are (pp. 6, 81). But in this day and age, questions raised by such nationalisms force themselves upon us and one can hardly resist the temptation to ponder them.

One central fact about nationalism as it exists is that it is not a uniform phenomenon. Rather than being a theory comparable to liberalism, it represents a class of theories, all of which share the definition of the political community as a "nation" (implying popular sovereignty and essential equality between members) and can differ dramatically in all other respects. All nationalisms satisfy and recreate a need for dignity, or (as Berlin, quoted by Tamir, defines it) status and recognition—which, in the absence of alternative ways to satisfy it, assures the continued appeal of nationalism. Different nationalisms, however, satisfy this need in very different ways and, in doing so, generate additional needs peculiar to them. In concrete experience, the abstract value of a "membership in a nation" is eclipsed by the value of a membership in a particular nation, with the particular satisfactions and comforts it affords. Being a member in a nation does not define one's identity: one is an American, a German, or a Russian—a member in the nation, defined as a community of free individuals, a superior race, a military superpower, a cultural giant, or the like. Very often, the satisfaction afforded by membership in the nation is predicated on the assumed inferiority of other nations, deemed to be undeserving of equal respect with one's own. Membership in a nation implies participation in the national ideals. These ideals differ: a supreme good for one nation may be an object of contempt for another. The nature of the national ideals defines the type and the specific character of any given nationalism. Liberal nationalisms are nationalisms based on liberal ideals. They are rare because illiberal nationalisms (of which there are many) are more effective in satisfying their members' need for dignity—perhaps because they relieve individuals of personal responsibility and define dignity as a collective attribute.

"Placing national thinking within the boundaries of liberalism" may, therefore, be equivalent to the attempt to combine different types of nationalism, subsuming an illiberal nationalism (e.g., the ethnic and collectivistic variety) under a liberal one and transforming the former into the latter, thereby changing people's ideals and aspirations. "Can this be done?" is a question of a great practical importance. Such a transformation of national identity, for example, is a necessary condition for the democratization of post-Soviet societies. Tamir constructs a philosophical ideal of nationalism, but in leading the reader to questions such as this, she also performs a valuable service for those who try to understand its reality.

Harvard University LIAH GREENFELD

Inequality. By Larry S. Temkin. New York: Oxford University Press, 1993. 352p. $49.95.

Larry Temkin, an associate professor of philosophy at Rice University, has undertaken a daunting task. Observing that philosophers have written scores of books and journal articles in recent years in defense of egalitarianism, what he finds missing from their collective enterprise is any discussion of how one might go about measuring the degree of inequality between alternative "worlds." While economists have discussed various ways in which inequality might be measured, philosophers have remained largely unaware of, or impervious to, their efforts. Temkin examines the work of the economists, but that is not his essential mission; rather, it is to apply a "Sidgwickian methodology" and the modus operandi of analytic philosophy to "explain, and ultimately guide, our egalitarian judgments," our "egalitarian intuitions" (pp. 5–6).

This mission—of determining when one situation is worse than another in respect to inequality—is a tricky, "complicated" business, one that requires scrutiny of competing principles of equality that have been promulgated by various schools of moral philosophy. These principles are often in conflict and thus lead to divergent assessments of how two competing "worlds" compare to each other. Temkin plays off these theories in myriad combinations, hoping to refine "our intuitions" about equality into "considered judgments." By distilling out equality from other desirable moral values, he hopes to understand it better. That understanding, then, can be factored into an "all-things-considered" judgment that takes other values into account. Only then can conclusions be reached about public policy.

Proceeding, as Sidgwick suggested, by beginning philosophical exploration with our ordinary moral judgments (both intuitions and underlying principles), Temkin seeks a coherent explanation of these "pretheoretical egalitarian judgments" that will enable us also to refine—and, where necessary, revise—these initial intuitions. He opens with a definition of who an egalitarian is—someone who cares about equality in itself, over and above its role in promoting other values. Excluded from the fold are some self-professed egalitarians whom Temkin calls "humanitarians." They are excluded because, while they favor transfers from the better-off to the worse-off, their sole objective is the improvement in the lot of the worse-off. Thus, they would be indifferent as between a regime of redistribution that took some "welfare" from the rich and gave it to the poor and a regime that made the poor and the rich both better off.

Inequality's intended audience—whose intuitions he counts on—is the "pure," "extreme" egalitarians—a by-no-means-insignificant cohort among today's moral philosophers. Their shared intuition, he argues, is that "undeserved inequality is always objectionable; whether or not it is unavoidable, any one is responsible for it, there is anyone for whom it is worse, or it involves different people, societies, places or times (p. 12). Temkin states that he will not defend this position: his purpose is elucidation, not justification. Other intuitions follow. It is a natural injustice for some to be born blind and others not: it is simply unfair. It is a social injustice for some to enjoy more *welfare* (left undefined) than others. Something needs to be done to remedy both sorts of injustice, because the social builds upon the natural. Those who are worse off have genuine complaints, and it is their complaints that matter. Temkin will try to measure and compare these complaints in various *worlds*.

Temkin's "impersonal teleological view of inequality" leads him to explore alternative "worlds" all exhibiting the same three basic characteristics: (1) the better-off are not responsible for the inequality that exists, either directly or through resistance to sharing their good fortune with the worse-off; (2) people are equally skilled, hardworking, and morally worthy; and (3) those who are worse off are in that position through no fault of their own. This should sound familiar; for Rawlsian counterfactual, "original position" theorizing has permeated moral philosophy in this country for over two decades.

With few exceptions (for cases of deserved inequality, which are acceptable but left undefined), these are worlds in which no one deserves anything, including talents and attributes. It is a universe in which useful things can be said about equality by comparing pairs of worlds, each represented by two rectangles varying in height depending on the welfare levels of two groups (the better-off, the worse-off) and varying in width by the number of people in each of the two groups. A goodly portion of the book is devoted to an examination of a sequence of 999 worlds that begins with a first world of 999 individuals who enjoy a high standard of *welfare* (left undefined but not just utility and not just measured in monetary terms) and one person who experiences a low standard of welfare. The world that occupies the median place in the series has 500 people at high welfare and 500 at low. The final world has one at high welfare and 999 at low. The question, to be probed at great length and in exquisite complexity is, Which world is better in "respect to inequality"? Does this sequence go from bad to good (because we are more attuned to inequality when it affects an individual or a small group), from good to bad (because some of our intuitions lead us to be more offended when more people are worse off), or from good to bad to good (because the first world deviates only slightly from equality, the middle world is the worst, and the progression from midpoint to the final world is on the upswing toward nearly perfect equality), or are all 999 worlds *equivalent*?

In examining these competing explanations of the sequence, Temkin considers "at least twelve different aspects or positions underlying or influencing our egalitarian judgments (there may be more)" (p. 50). It is a testament to the author's diligence that he kept count. The reader is treated to a richly textured (or stultifying, depending upon one's tastes) exploration of the interplay among a *maximin* principle of equality, an *additive* principle, a *weighted additive* principle, a *relative-to-the-average-view-of-complaints* principle, a *relative-to-the-best-off-person* principle of complaints, and a *relative-to-all-those-better-off* principle of complaints, among others.

Temkin makes few absolute judgments, preferring a "some people might oppose/many would claim" approach. However, the judgments that he does make seem dubious at best. Since he does not provide knockdown arguments in defense of any of his judgments, they are difficult to criticize and even more difficult to accept if one does not share the author's initial intuitions about equality. Some of these judgments are that if improvements to the better-off can be made without worsening anyone's welfare, this would be bad, whereas worsening the better-off without aiding anyone else would be good (p. 87); that egalitarianism is comparative, so that an egalitarian has "no intrinsic concern with how much people have . . . [but] with how much people have *relative to others*" (p. 200); that the slogan "One situation *cannot* be worse (or better) than another if there is *no one for whom it is* worse (or better)" deserves to be seriously challenged; that in the 999-world sequence, the worlds first get worse then better; and that for any two reciprocal worlds in the sequence, "that one will be worse whose better-off group is *bigger*" (p. 297, my emphasis). The last claim means that the last world is better than the first: it is preferable to have 999 people at low welfare and one at high than 999 well-off and one not.

These last two claims about the sequence are "pretty uncontroversial," Temkin thinks, and he simply "assumes that they are correct"; this lack of argumentation at such critical junctures is rather maddening in its own right, but even more so when, on the very next page, in an example supposedly illustrating the claims, one reads that "between reciprocal worlds, that one will be worse whose better-off group is *smaller* (pp. 297–98; my emphasis). This contradiction is undoubtedly the result of inadvertence; but there is a deeper problem here, namely, that one reader at least cannot state with conviction which view Temkin endorses—and this discussion is in a concluding section labeled "Practical Implications." Here, too, Temkin seemingly disregards his own repeated imprecations that "some of the conclusions reached may not be generalizable to the real world" (p. 29) and proceeds to generalize without any indication of how equality ought to be weighed against other, often competing, moral values. Thus, we need to rethink our quest for equality of opportunity, because it will lead to greater inequality by producing a meritocratic society with the talented of all races and genders winning the social, as well as the natural, lottery.

Inequality concludes with a declaration that our "pretheoretical intuitions need revision in light of moral theory" (p. 302). Yet one is left with the nagging thought that perhaps it is the moral theory itself that needs revision. For nonegalitarians, the vexation goes even deeper, to the very intuitions that undergird the entire project. Why set up an idealized, counterfactual world in which no one is responsible for anything, no one deserves anything? (When economists theorize from an abstraction of human nature, the model, representing people as self-interested maximizers who know their own interests and prefer more utility to less, is at least recognizable. That is why the deductions of economists bear a relationship to how people actually behave in markets. Beginning from counterfactual assumptions, it is also not surprising that Temkin's mission, as it progresses, moves further and further from reality.) Why assume that what matters to people is comparing their welfare to the welfare of others? Why model the world in a way that gives central position to complaints, that is, to envy? Why should we care about equality in itself, irrespective of human welfare?

Political scientists, I suspects, will find this book bewildering. Particular aspects of the book will concretize our dismay. Chapter 3 is one example. The author conducts an "informal poll" by sending a questionnaire to 40 colleagues of his at the National Humanities Center, receives 10 responses, and then defends the sample as sufficient because the results cohered with those he achieved in the balancing-of-the-twelve-principles chapter. Chapter 10 provides another example. In discussing how our world relates to the worlds in the sequence, the author argues that our world resembles one of the later worlds, and to demonstrate this he cites sets of figures on inequality that are between two and three decades old. In an aside in the accompanying note, this lapse is explained: "Though . . . the particular data cited, is now somewhat dated, the basic pattern of inequality it suggests is unfortunately, not" (p. 302).

These two examples illustrate, one supposes, why philosophers are philosophers and not social scientists: all of the former's instincts draw them to the abstract, the otherworldly, the metarealm; all of the latter's inclinations lead them to the concrete, the empirical, the measurable (or at least, the observable). Thus, what precedes is the rather skeptical—some might say unduly harsh—assessment of a social scientist.

Bowling Green State University ELLEN FRANKEL PAUL

From Politics to Reason of State: The Acquisition and Transformation of the Language of Politics, 1250–1600. By Maurizio Viroli. New York: Cambridge University Press, 1992. 331p. $54.95.

In 1612, Trajano Boccalini took a step beyond Giovanni Botero to do what would have been shocking and unspeakable to Brunetto Latini in 1266 or even to Niccolo Machiavelli in 1512. He gave to "reason of state"—that insidious art of preserving the state of a prince—the good name of "politics." To these earlier writers, the word *politics* picked out the art of ruling a republic according to justice and reason—however necessary it might be, given Italian realities, to read or write those very different discourses on the means of preserving a state. A revolution in "politics" had plainly occurred. Hereby hangs a tale of ideological innovation and conceptual change told with considerable erudition and detailed scholarship by Maurizio Viroli.

Across four centuries of Italian writers, Viroli narrates the acquisition and transformation of the language of politics. He begins in the late thirteenth century, when the linguistic traditions of political virtue, Roman civil law, and Aristotelianism provided writers on city government the semantic resources to conceptualize politics as the art of the republic. In the next century, the civic humanists completed the elaboration of the idea of politics as civil philosophy, glorifying it as the most excellent human endeavor. In their different ways, Machiavelli and (especially) Guicciardini gave unprecedented theoretical power to the upstart *art of the state,* but without conflating this art with the earlier and more noble one to which they (especially Machiavelli) also contributed. The last glimmerings of civil philosophy are seen in the later sixteenth century at precisely the time that the art of the state was elevated to *reason of state* and, soon enough, to *politics* itself. Attempts to restore the noble art of the republic proved impossible or in vain. The triumphant reason of state made of it "a language of nostalgia or utopia": "The story ends where it began: the same country where the language of politics as civil philosophy was born, nourished also the growth of its mortal enemy and grave-digger" (p. 280).

From Politics to Reason of State is an exemplary history of political thought and case study in conceptual change, executed roughly along lines charted out by Quentin Skinner, to whom the book is dedicated. Bidding us to leave aside our contemporary mental habits and linguistic usages, Viroli painstakingly excavates earlier languages of politics and the state through close textual readings informed by an impressive knowledge of different conceptual contexts. An imposing list of theorists (most of them heretofore unsung) is analyzed with care and clarity. Indeed, most readers will find (or take it on good faith) that the list of theorists is exhaustive; and only a few will find it a little exhausting. Besides its intrinsic merits, the story that Viroli tells also provides him with the material in the epilogue "to prefigurate a conception of politics to which it is worth committing ourselves" (p. 10), namely, a restoration of civil philosophy itself.

In an otherwise excellent chapter on Machiavelli as master of the arts of both politics and state, Viroli sides with the view that the purposeful author of *The Prince* was attempting to educate "the one great political man" and perhaps engaging in a bit of "self-promotion," to boot (pp. 153, 177). But the case for this is less well argued than it should be, given the detailed linguistic work Viroli has put on display. The Medici lord to whom Machiavelli dedicated and submitted his treatise was not a "political man" (in his sense): he was a corrupt grasper after his state. Viroli notes how the advice on patronage is "unconventional" and "deliberately distorted," whereas that on the utility of fortresses is downright "subversive" (pp. 149, 151). Yet Viroli takes little advantage of his own tempting remarks, even after he goes on to acknowledge that in the *History of Florence*, Machiavelli disguisedly remained the "implacable opponent" of the Medici (p. 168). Whatever linguistic refinements he made on the concepts of politics and state, what Machiavelli the political man (in his sense, and ours) was doing in writing *The Prince* is still rather a mystery.

Mysterious, too, is how we can actually restore civil philosophy in our time. Having presumably been buried by reason of state in the seventeenth century, at least in Italy, civil philosophy's twentieth-century resurrection as "the art of the city" is a bit breathtaking. What sort of art, in the era of economics and the policy sciences? What sort of city, in the era of nation-states and transnational corporations? What will make civil philosophy sound like more than a few nostalgic sentiments about justice and friendship? Given such questions, it is surely very "strange" (as Viroli admits) to hear that such an art might form a discursive alliance with "the heirs of Kant and Mill," rather than "the contemporary advocates of republicanism" (p. 192). A historically informed civil philosophy may indeed provide rhetorical and theoretical resources to criticize statists, realists, participatory democrats, and communitarians. But one is tempted to ask exactly how civil philosophy and modern liberalism can cohere, cognitively and practically. After Viroli's careful linguistic work, one is tempted to ask how they can cohere "politically," as well. Worthy as the goal is, saying so will not make it so. Everything remains to be worked out. In the meantime, however, there is no doubt that "politics" in early modern Italy has received a first-rate chronicle. The memory it revives may yet assist the restoration of civil philosophy in our time.

University of Minnesota JAMES FARR

The Problems of a Political Animal: Community, Justice, and Conflict in Aristotelian Political Thought. By Bernard Yack. Berkeley: University of California Press, 1993. 309p. $45.00 cloth, $14.00 paper.

Bernard Yack's new book is an incisive and well-written meditation on the inescapable conflicts that define human communities. Unfortunately, his title is a bit misleading, since the word *problems* implies that there are solutions, whereas the whole point of his book is to show that the tensions and dilemmas of communal living are unresolvable. Moreover, his concern is not limited to political communities but includes families, friends, business partners, and even the passengers on a ship. Yack develops an Aristotelian theory of moral and political life in which conflict is seen not as the breakdown—but as the expression—of communal bonds. What this means is that the intensity of conflict is proportional to the closeness of the bonds—which helps to explain why, for example, police officers are often terrified of domestic disputes. Yack uses this dialectical unity of conflict and community to explore a variety of major themes in Aristotle's thought, including teleology, friendship, justice, the rule of law, class conflict, and the good life. In every case, his mode of analysis brings valuable and interesting aspects of Aristotle's thought into focus.

Yack carefully positions his argument in relation to individualist and communitarian interpretations of political community. Indeed, his book is as valuable as a guide to the current interpretative debates about Aristotle as it is to the writings of Aristotle. Against the individualist view that community is merely an instrument for pursuit of an individual life plan, Yack argues that we are by nature communal animals, whose identities are largely the product of our social relations. But against the communitarians, he insists that community is not the locus of harmony and integration but of conflict and division. And against perfectionism of all stripes, Yack insists that our conflict-ridden communities are an inexorable part of the human condition: there are no transpolitical ideals toward which politics can progress. To use Oakeshott's image, the only anchor available to the ship of state is a "sea" anchor. The domains of conflict may change over time but not the nature of conflict. Finally, against all kinds of nostalgia for the alleged harmony of the Greek polis, Yack insists that we are fundamentally in the same contentious political boat as Aristotle's Athenians (which is why Aristotle's relections on politics never lose their relevance).

Yack's Aristotle might be called a "Hobbesian communitarian" because of his paradoxical emphasis on both conflict and community (though this is equivalent to saying that Aristotle is neither a Hobbist nor a communitarian). According to Yack, the debate between individualists and communitarians remains wedded to the superficial contrast of society (*Gesellschaft*) and community (*Gemeinschaft*): modern, or urban, society is the locus of exchange relations among competing individuals, while traditional or rural community is the locus of shared moral bonds. Drawing on the sociological insights of Durkheim and Simmel, Yack blurs this contrast by showing that the exchange relations of society presuppose communal norms of good faith, while the shared moral bonds of community are often the product of exchange and other kinds of reciprocity. Aristotle did not need Durkheim, says Yack, to teach him that human community is built from the complementarity—not the similarity—of its members. Beginning with marriage, we form communities with individuals who are different from us, so that through division of labor and exchange, we may benefit from the pooling of individual talents. Who wants to go to a picnic where everyone brings the same dish?

But if difference is the precondition for community, it is also the precondition for conflict. Because of our differences, we have different views of the ratio of contributions to rewards in the give-and-take of communal life. In the absence of a common metric by which to measure the different kinds of contributions to the common good, there is no calculus to determine the right distribution of rewards. (Yack does not evaluate the claims of libertarians that the market effects such a calculus.) The conflict over distributive justice is bitter to the extent that communal bonds are strong, for we are most likely to feel betrayed by those to whom we are closest.

Yack's emphasis on the conflictual dimension of political community is especially revealing in his analysis of class conflict. Yack (following Finley and others) laments the conflation of all class struggle with Marxian class struggle. He defines Marx's class struggle as conflict between "groups of individuals who are hierarchically positioned within the process of production" (p. 211). Unfortunately, this does not do justice to Marx's theory, because, on this definition, one would expect the class struggle of feudalism to be between aristocratic landowners and peasants, whereas, according to Marx, the struggle was between the aristocracy and the bourgeoisie. These latter two classes represent not "groups hierarchically positioned within the process of production" but declining and rising modes of production, or the country and the town. Still, Yack's own interpretation of class struggle (especially ancient Athenian class struggle) does not depend on his interpretation of Marx. Yack argues, on Aristotelian grounds, that class conflict is most common among citizens, not between status groups: in ancient Athens, masters and slaves rarely fought, because they had little in common, but rich and poor citizens fought often, because they had political life in common. Each class felt betrayed by the other class, with whom it shared the political arena. Political friends make for class enemies.

Because politics has always been so conflictual, there has always been a longing for an escape from politics—a "final solution" to the "problems of a political animal." Yack explored some strategies for such escape in his first book, *The Longing for Total Revolution* (1986). There are several possible escape routes. The first—the quest for communal harmony—we have seen to be chimerical. Another, according to Yack, is legalism—the attempt to set forth a code of laws, principles, and imperatives, based on human nature or human reason, that purport to define what justice once-and-for-all requires. Yack's Aristotle defines politics not as an attempt to embody any set of abstract principles but as the conflict over those principles—principles that are "essentially contestable." Thus, Yack (following Harry Jaffa and others) strongly distances Aristotelian natural right from any kind of natural law. Yet he acknowledges a kind of procedural natural law principle of justice when he says, "Nature thus tells Aristotle *who* can participate in deliberation about political justice but *not what* they should choose" (p. 149). Since nature endows human beings with reason and speech (in order to discriminate between the just and the unjust), by nature, all rational agents should be allowed to participate in politics. But Yack denies that Aristotle has any natural law principles to decide substantive questions of political justice: when it comes to such principles, we are on our own. Aristotle, in short (unlike Rousseau, Marx, and Nietzsche), never yielded to the temptation to suggest an escape from politics.

Aristotle says that in order to become a good person we need three things: nature, habit, and reason (*Politics* 1332a40; see also *Nicomachean Ethics* 1179b20). We begin with our natural aptitudes; these are then shaped into habits; and finally, we use our reason to reflect upon our habits and to form new habits. At the political level, excellence requires the right human nature (shaped by eugenics), the right customs, and the right laws. Habits or customs presuppose nature but cannot be reduced to nature, while reason presupposes habits without being reduced to habit. In their accounts of Aristotle's ethics, different scholars emphasize one or another of these three elements. Roger Masters, in *The Nature of Politics* (1989), emphasizes human nature; John Finnis, in *Natural Law and Natural Rights* (1980), emphasizes reason; and Bernard Yack emphasizes the social inculcation of habits. Each account is thus equally valid and equally one-sided. What is needed is an account of Aristotle's ethics and politics that not only includes nature, custom, and reason but also shows how each element is related to the others in Aristotle's dynamic model of human flourishing.

Dartmouth College JAMES BERNARD MURPHY

AMERICAN POLITICS

Real Choices, New Voices: The Case for Proportional Representation Elections in the United States. By Douglas Amy. New York: Columbia University Press, 1993. 278p. $32.50 cloth.

Restoring Real Representation. By Robert C. Grady. Urbana: University of Illinois Press, 1993. 170p. $29.95 cloth.

As the titles of both books indicate, these works set out to make a case for improving representation. Although both do a fine job of laying the groundwork of their theses, they make a less than convincing case that their proposed reforms would in fact *improve* the quality of representation.

The key shortcoming of both books is that the authors overlook—or at least do not address—a key lesson to be drawn from Hannah Pitkin's *The Concept of Representation* (1967). Representation is a complex, multifaceted notion which has several manifestations which are not necessarily mutually compatible. Both Grady and Amy fail to acknowledge, despite the earnestness of their endeavors, that they have merely taken stands at and in favor of different points along a representational continuum. Thus, while both books' arguments for change provide ample food for thought, readers are likely to be left wondering if the proposed changes would produce the benefits that the authors contend.

Grady begins by lamenting the "displacement of popular representation in favor of interest group representation" (vii) which is symptomatic of interest group liberalism (p. vii). He seeks in this admittedly polemical work to "make participation and popular representation once again central to governance" (p. 5).

Contending that the current state of liberal democracy diminishes the value of popular participation and civic responsibility, he seeks to remedy this as well as the lack of government accountability that characterizes interest-group liberalism. To do so he develops a vision of political participation which is a hybrid drawn from the confluence of participationist theory and corporatism "by combining corporatist functional jurisdictions, which interact with public officials and complement the roles of elected legislatures, and participatory practices within such jurisdictions" (p. 8). While retaining the corporatist structure, he extends his participationist vision to include other interests or private associations all of which are grouped under his heading, *democratic functional constituencies (DFCs).*

If all of his conditions were met, Grady's vision would certainly lay a coherent theoretical groundwork for a system in which voters would be provided with more incentives and opportunities to participate meaningfully in governing and, as a result, become better citizens. In Grady's vision, the government would delegate authority to DFCs (which, by definition would be democratically run) and do so with strict guidelines which would limit the discretion that the DFC leadership could exercise in pursuing a given governmental end (p. 142). Such an arrangement would enhance the tie between the government and the people who participate in various DFCs while ensuring that those who worked with the government would not behave in the egoistic manner that characterizes interest-group liberalism.

A key proviso is that such a delegation would, by definition, be in "the public interest." It is here that Grady's argument becomes harder to follow. Insofar as he distinguishes between the public interest and the desires of the organized interest groups, Grady seeks to ensure that these discrete interests would be subsumed by some greater public interest which would be determined by legislative deliberation and made manifest in the clear guidelines which would accompany any delegation to a DFC.

This begs the question: from where would these public-spirited legislators emerge? In part, Grady's answer is implicit: the beneficial impact of workplace democracy in DFCs would manifest itself in the voter's choice of legislators. However, Grady never really explains how citizens, who would apparently define their vision of the political in terms of their DFC activities, would separate their DFC agenda from the public good when it came time to elect a legislator. As well, Grady himself admits that, somehow, legislators and DFC leaders would need to be coaxed away from behaving as they do under interest-group liberalism in order to act instead in the name of some public interest. He does not, however, explain how feasible or likely this would be.

This begs a second question: How would the truly *public interest* be discerned? Insofar as the legislators' restrictions would be made clear in a delegation of authority, their vision of "the public interest" could be made manifest. However, since Grady does not define what would count as truly public-spirited legislative deliberation, he offers no measures for determining whether or not the legislature was actually behaving.

Clearly, Grady places a premium on participation as the groundwork for a more legitimate governmental process. Yet he does not address whether mere participation is in fact tantamount to having one's interest *represented* more effectively. Granted, Grady's work seems premised on the assumption that with time the public interest would become clear to all and that individual interests would succumb to or be included as part of it. In this respect, the notion of representation qua the voicing of or "standing for" particular and discrete interests would be meaningless since the *public* interest (or at least an enlightened self-interest) would, ideally, become a principal motivation for more and more political action.

Nonetheless, to the extent that legislative deliberation would be a key determinant of what the public interest would be at any given time, Grady's vision includes the possibility that even under his ideal conditions of public-spiritedness and participation, there would be competing views of the public interest. As a result, certain interests would lose out insofar as their elected representative might not agree with the legislative majority's deliberations.

Thus, to the extent that individual participation can be regarded as a surrogate for if not a replacement of interest representation, Grady's thesis is intriguing. Nonetheless, the question of whether mere participation alone is sufficient to achieve satisfactory representation of interests remains an issue. This same problem also arises in Amy's discussion.

In seeking to make the case for America's conversion to proportional representation (PR) as a first step in curing the principal ailments of the political system, Amy begins with the assertion that "the American election system is unfair, outmoded and undemocratic" (p. 1). The system would improve if: gerrymandering were eradicated (chapter 2), campaigns were more issue-oriented (chapter 3), we broke the two-party monopoly (chapter 4), we elected more women and minorities (chapters 5 and 6) and more people participated in elections (chapter 7).

He explains that eradicating gerrymandering would minimize wasted votes. Issue-oriented campaigns would provide more meaningful choices to voters and PR would encourage parties to be more issue-oriented by removing the need to garner a majority of the votes in a district to win seats and legislative influence. Increasing the number of parties and increasing party discipline would also enhance the issue-focus and quality of voter choice. The election of more women and minorities would make legislatures more reflective of the population. And higher turnout would legitimate the government. This said, one cannot help but react with skepticism when Amy suggests that the root of evil is the single-member plurality system of voting (SMP) or that a conversion to some form of PR will solve all of these problems.

Proponents of PR have always contended that gerrymandering would become less of an issue because PR would create the possibility for multimember districts. It would also appeal to the aesthetic desire for a more accurate translation of votes into seats in any given election. The issue, however, of whether a conversion to PR would result in qualitative improvements in representation remains the source of an ongoing debate. Absent strong empirical evidence to support many of his contentions, Amy's argument in favor or PR remains quite speculative.

Amy himself admits at several points that his assertions are not grounded in empirical evidence. He argues, for example, that PR would lead to more issue voting (p. 70), yet he says forthrightly in an accompanying footnote that there is no empirical evidence to support this claim. Similarly, his reliance on Blais and Carty (1990) to sustain his assertion that PR would increase voter turnout is, as he points out, qualified by the fact that they could not explain *why* countries that employ PR systems tend to have higher turnouts than those with SMP.

Even if a conversion to PR were to bring about some of the changes that Amy suggests, whether or not it would result in representational improvements remains the focus of a debate that has continued for some time. While a more accurate translation of votes into seats in any given election might have a certain aesthetic appeal, PR does little for a minority if, having been shut out of legislative representation due to discriminatory laws, a particular electoral system or gerrymandering, it now has the privilege of seeing its interests or agenda defeated in the legislative bargaining process in proportion to it numbers.

Calhoun dealt with this over a century ago in his *Disquisition on Government* and Lani Guinier has echoed Calhoun recently in "The Triumph of Tokenism" (1991) and "No Two Seats" (1991). Although both Calhoun and Guinier challenged the mathematical unfairness of SMP, both also acknowledged that mere proportional representation would do little more than establish a token presence in the legislature unless the minorities were given some sort of veto power which, for all intents and purposes, would give them *disproportionate* influence in the legislature. Amy, however, cites neither critic and, as a result, his contention that PR would be a panacea for all that ails SMP systems stands shaken.

Doubtless, Amy's book serves to reassert that there are indeed two sides to the PR-SMP debate, that both are grounded in thoughtful concerns about effective representation and that, perhaps, the debate still needs to be hashed out in the United States. In *Real Choices*, however, Amy does little more than cast his lot with PR's proponents. SMP may have its shortcomings, but it certainly has the support of many eminent scholars. He dismisses these concerns much too quickly. While devoting some 190 pages to advocating PR, he dispenses with the objections to it in a mere 30 pages. The result is that his approach to the issue lacks balance and the book reads more as a thoughtful polemic than an objective analysis.

In sum, both books are provocative, yet ultimately they fall short of their goal of setting forth convincing schemes to improve representation. Insofar as the representation debate has expanded beyond the notions of simply ensuring participation or obtaining a proportional presence (instead of some sort of proportional *impact*) in the legislative process, both Grady and Amy have added support to the respective aspects of the debate upon which their works focus. Both works, however, leave many questions unanswered.

Washington and Lee University MARK E. RUSH

The Rebirth of Urban Democracy. Edited by Jeffrey M. Berry, Kent E. Portney, and Ken Thomson. Washington: Brookings Institution, 1993. 344p. $36.95 cloth, $16.95 paper.

This is an earnest book and a good one. The authors seem torn between the optimism about neighborhood-based participation that is reflected in the book's title and their responsibility as social scientists to provide solid data and evaluate it with measured skepticism. While Berry and his colleagues occasionally lead with their hearts, their book offers a rich data base and serious analysis anchored in important concepts about power and democracy.

Much of what has been written about neighborhood organizations in the past has been thick with ideological celebration (leftist visions of a mass movement against the prerogatives of capital or rightist salutes to the ability of citizens to help themselves without governmental aid) but empirically spare. The authors provide an antidote to that trend. Their central theoretical issue is whether expanding citizen involvement leads to frustration, disorder, and delay (a prediction they associate with Joseph Schumpeter and Samuel Huntington) or to more satisfied, knowledgeable, and civic-minded citizens (as would be predicted by Rousseau, Mill and contemporary communitarians). Their empirical focus is on five core cities (Birmingham, Dayton, Portland, St. Paul, and San Antonio) selected because preliminary research showed them to have unusually inclusive and effective mechanisms for citizen participation. In each core city, the authors surveyed about 1,100 residents, then reinterviewed most of them about a year-and-a-half later. A single wave of surveys was also carried out in 10 comparison cities chosen to match the core cities in size, race and class. In addition, in each of the five core cities, the authors conducted interviews with from 77 to 106 administrators, council members, neighborhood and community activists, and other political elites.

Perhaps the most surprising finding concerns the relationship between structured participation channels and actual levels of citizen involvement. Using a broad measure of community participation, the authors found that citywide structured systems for citizen participation did not generate higher levels of activity than were found in the comparison cities. Indeed, one of the core cities, San Antonio, had substantially lower levels of participation than any of the others; Dayton and Birmingham also scored in the bottom five out of the 15 cities. This finding led the authors to restructure the rest of their analysis. Rather than simply compare the five core cities to the others, they categorized all 15 cities based on two dimensions: the overall level of citizen participation and whether participation mechanisms were formally structured. This shifting of gears makes the analysis muddier and less well defined in places, but it allows for some interesting distinctions and comparisons. For example, compared to the cities with unstructured participation, the five core cities tend not to do as well in stimulating involvement by low socioeconomic-status and minority citizens. But there is some evidence that the structured cities have a more positive impact on the sense of political efficacy and levels of political knowledge of those low-income and minority citizens who *do* participate.

The authors' predisposition in favor of structured participation mechanisms reveals itself in their choice of a frame of reference. In much of the book, they are content to strike blows against the belief that expanded participation will lead to elite dominance, intolerance, conflict, parochialism, and governmental paralysis. They make a convincing case that these fears are not justified. Neighborhood groups tend to serve as a counterbalance to business interests, to teach participants to be more tolerant of others, and to stimulate a sense of community. Public officials and nonparticipation citizens see these groups as legitimate actors that facilitate negotiation and compromise and, ultimately, better policies. Some readers however, will feel that the authors have delivered most of their blows to a straw man. The fear that participation will unhinge democracy is less common today than it was in the 1950s or 1960s, and its focus has never really been on the kind of structured, formally recognized, genteel and civilized forms of participation that characterize four of the five core cities considered here. (San Antonio's genre of participation is more free-wheeling and contentious; on the indicators that the authors care about, however, it does not perform nearly as well as the other cities).

A more reserved reading of the evidence they present might lead to a less exuberant endorsement of neighborhood associations than Berry, Portney, and Thomson end up with. Some of the findings they cite to discredit the notion that participation is destabilizing can also be read as evidence that these elaborate structures do not have very much impact one way or the other. They find little evidence that neighborhood groups are a positive force in setting the local agenda; and they concede that neighborhood associations can rarely stand up to business when it comes to development issues of any size or of citywide consequence. Because they make no effort to quantify the cost of sustaining these institutions, they are in no position to conclude whether the small positive effects they uncover represent a decent return on the investment. Given the fact that they began their study by selecting cities that seemed to be models of success (the authors acknowledge that there were "few other cities that even came close to the five cities in the scope of their neighborhood-based citizen participation" (p. 294) the absence of more striking differences between the core cities and the comparison group is instructive.

It is premature to declare that the establishment of citywide, neighborhood-based structures for participation will bring about a rebirth of urban democracy. But this book is sufficiently sound and sufficiently stimulating to lead us toward a rebirth of interest in the topic as a focus of public debate and as a subject for scholarly investigation.

George Washington University JEFFREY R. HENIG

Inviting Women's Rebellion: A Political Process Interpretation of the Women's Movement. By Anne N. Costain. Baltimore: Johns Hopkins University Press, 1992. 188p. $28.00.

In less than 200 pages, Ann Costain analyzes the contemporary women's movement from roughly the 1960s through what she describes as its crescendo in 1975 to its current state of apathy and decline. She uses interviews and secondary archival research, as well as content analysis of news stories and official documents. The author is to be commended for integrating political process theory from sociology into political science. More specifically, she has made noteworthy contributions to the literature in establishing linkages between external political forces and ultimate governmental response.

Costain argues that the debate over what constitutes a social movement can be isolated into three competing schools of thought; *mass society, resource mobilization,* and *political process.* She sets the stage for her study by informing the reader of the theoretical development of the political process approach, which, according to Costain, grew out of resource mobilization theory. However, as opposed to resource mobilization theory, which concentrates on resources (e.g., entrepreneurial organizers), the political process approach places more emphasis on the political system, "indigenous accumulation of resources, and psychological aspects of movement identification" (p. 12).

The mass society theory, which characterizes William Kornhauser, Philip Seiznick, and Hannah Arendt's research into early twentieth-century European industrial societies, was according to Costain, applied to American politics in the early 1950s by Seymour Martin Lipset, Daniel Bell and Richard Hofstadter and, still later, by Ted Gurr in his *relative deprivation* variant. Costain then goes on the trace the development of resource mobilization theory formulated by sociologists in the 1970s to challenge mass society theory and relative deprivation (p. 6). According to Costain, resource mobilization theory suggests that the principal problems confronting most movements can be traced to organization and the task of starting up while concurrently increasing new membership and retaining existing members. Costain cites the work of Robert Salisbury and his ideas about exchange in identifying ways that movements succeed. She feels that most research to date on the women's movement has adopted this perspective. She references the work of Gelb, Palley, Mansbridge, Conover, and Gray as being premised on resource mobilization. Costain also points to Jo Freeman's research, using relative deprivation, as work that also linked resource mobilization theory, in that it combined Gurr's work (women comparing their jobs and social prospects with men of the same age and educational background felt unfairly treated) with an early emphasis on the resources of communication networks needed to organize and spread information about the women's movement (p. 6). The author argues that the major implications of resource mobilization theory, as applied to the women's movement, are that the movement emphasized its nonthreatening characteristics (appearing politically innocuous) and that an early accumulation of resources followed from this focus (p. 9).

Costain feels that resource mobilization theory fails to explain early issues surrounding the women's movement, such as the public protest of the movement, the concerns of the early lobbyists for women's rights in the late 1960s and early 1970s, the larger questions of why women actually organized a social movement, the movement's rejection of hierarchical leadership (thus eliminating the most obvious means to accumulate resources), and the lack of large numbers of supporters of the movement in its early years.

On the other hand, Costain posits that a political process theory can more accurately explain the development of the women's movement. In this approach, for a movement to emerge, its followers "must believe that problems have political roots and that they can do something to change existing conditions. . . . Most important of all, the government must at least be willing to tolerate the movement's appearance" (p. 12). Costain goes on to build on the work of Ethel Klein (*Gender Politics*) and others to test the two approaches empirically. Costain does so by coding events reported in the *New York Times Index* for the period 1950–86, content-analyzing other secondary sources (papers of the National Organization of Women [NOW] and WEAL, the *Congressional Record U.S. Statutes at Large*) and interviewing lobbyists for women's groups and members of Congress and their staff. Her interviews spanned intermittent periods from 1974 to 1984. She uses discriminant analysis to assess how much influence region, political party and liberalism had on the individual decisions of members of Congress about whether to sponsor the Equal Rights Amendment (ERA). This was performed for each of the five Congresses in session (the Eighty-eighth through the Ninety-Second).

Costain addresses such complex issues surrounding the women's movement in order to include the disunity among women's groups, the pro- and anti-ERA backers, the influence of such nationally organized groups as NOW and NWPC, the concurrent weakening of the New Deal electoral coalition and politicians' recognition of the female voting power, the influence of the chief executive and his leadership in the movement, and the downfall of the ERA and success of the Reagan administration in moving women's rights from an active to a largely symbolic agenda item (p. 120).

The author concludes that as women's groups marshalled their strength , Congress took the lead in boosting the legislative fortunes of the ERA: "As the ERA headed to the individual states in the fight for ratification, it continued to provide the prime catalyst that united and energized the women's movement" (p. 78). She argues the peak of the women's movement coincides with the high point of media attention given to the ERA and a sharply concentrated focus on legal equality as a solution to women's concerns. Costain feels that these equality issues triumphed over the alternative agenda emphasizing special needs, or needs that government should address because of biological, physiological, and familial differences: "This new predominance of equality issues over special needs was less a choice of the women's movement than of the legislative and executive branches of government. . . . However, the increasingly single-minded focus on the ERA set the stage for a decline in the women's movement visibility in the 1980s" (p. 99).

Costain recognizes the problems inherent in establishing linkages between external political forces and ultimate governmental action as a result of this political pressure: "Discerning the balance between government response to outside pressures and internally generated initiatives on women's issues is more difficult than it first seems" (p. 42). She also recognizes the problems that scholars have encountered in using content analysis as a method for establishing this linkage. For example, she mentions the bias in the higher probability of "hard news," including demonstrations, riots, and other events to the exclusion of "soft news," including political speeches and other more analytical exchanges. However, she does not fully explain her motivation for excluding editorials, letters to the editor, comments, and other opinion-type pieces. She does point out: "Most scholars who employ events data exclude from event totals all editorial or other *New York Times*-generated stories, such as retrospectives on the movement or Sunday magazine articles. That practice was also followed here to cut down on the extent that *New York Times'* editorial judgments might skew the results" (p. 15–16).

However, since Costain relies primarily on analysis of secondary sources for much of her interpretation of the movement and for her efforts to link newspaper accounts and other documents to the political process, it would seem that some sort of analysis of editorials and thought pieces to accompany her events analysis would have enhanced, and added further credibility to, her study. This would especially appear to be the case in establishing linkage between the key political players she interviewed, what the newspaper was concurrently reporting, and the ultimate fallout from the political process. For example, would not the editorials and other opinion pieces have further reflected the movements' success and/or failure in setting an agenda item for government leaders to consider? This would seem to be the case especially because large numbers of governmental leaders acknowledge the influence of the *New York Times*. For example, the question arises, "Was the ERA the leading issue reported by the *New York Times* because of its significance for equality for women in our society or because the news media tend to focus on events, as opposed to issue analysis?" (see p. 79). Also, especially with regard to the linkage aspect, it would appear that her study would have withstood closer scrutiny had Costain also related her findings from her content analysis more specifically to the information gleaned from her interviews. For example, how were members of Congress and lobbyists specifically influenced by the events reported at the time? What role did they see themselves playing in the women's movement? What were their specific observations at the time with regard to equality for women? I wish Costain had used her interviews extensively to establish clearly just how the government facilitated the movement's success and, specifically, how pressure from various groups brought about this political change.

Nonetheless, Costain's research answers more questions than it leaves unanswered. Her work is clearly organized and written, and she makes a significant contribution to the existing literature. Finally, her research succinctly portrays the successes of the women's movement while summarizing valuable lessons learned along the way. This should be invaluable for future pursuits geared to improve the legal, social, cultural, economic, and political well-being of the American female.

Clemson University LOIS LOVELACE DUKE

Legislative Leviathan: Party Government in the House. By Gary W. Cox and Mathew D. McCubbins. Berkeley: University of California Press, 1993. 324p. $45.00 cloth, $14.00 paper.

The conventional wisdom is tenacious. Political parties are weak in the United States. Indeed, congressional politics can be understood without factoring party cleavage or polarization into the analysis because the parties in Congress have declined. Or, congressional parties can simply be ignored, omitted from institutional analysis by assumption or stipulation because they are so ineffectual. Congressional politics are "committee-centered," gravitating toward autonomous committee work groups providing "subcommittee government" to the institution. Party voting is paltry, and in the event reflects merely the inexorable influences of largely incidental ideological or attributional commonalities among constituencies. Party organization is trivial, and party leaders are really only followers of the central preferential tendencies endemic within their party cohorts. Representatives care not for loyalty to their party, but only attend to their reelection the next time around. Put in its baldest form, the conventional wisdom about Congress truly is perverse.

Cox and McCubbins charge into this arena with genuinely stimulating argument and analysis. They argue that both the "traditional" and the "rational choice" views of congressional party politics are deficient. The traditional perspective overworks the extent of committee and subcommittee devolution of power, and underplays the coordinative leadership of congressional parties. The rational choice view, all too often, assumes political parties out of the model altogether. In contrast, say Cox and McCubbins, a careful analysis of the committee structure permits a variety of inferences indicating the existence of party government in Congress.

The authors' work entails manipulating an impressive variety and quantity of data. Their purview for testing claims rests upon the House of Representatives. Most heavily relied upon are data for members' committee assignment and transfer requests from the 80th (1947–48) to the 100th (1987–88) Congresses. Ideological assessments are drawn from Americans for Democratic Action (ADA), Americans for Constitutional Action (ACA), and other interest group ratings developed from House roll-call voting. In addition, the authors make use of *Congressional Quarterly's* conservative coalition support scores, and the so-called NOMINATE scores (undimensional scale scores derived from all non-unanimous roll-call votes). A piece of the analysis relies upon four decades of congressional election data (1948–1988). The authors also use House roll-call voting from 1933 to 1987 to construct party leadership support (loyalty) scores for members for each biennium. Finally, selected data were gathered on bill sponsorship, and on majority and minority opinions in committee reports. To make a long story short, Cox and McCubbins' findings rest on analyses of substantial and impressive sets of data.

The principal findings from this research can be encapsulated blow-by-blow. (1) The House committee-assignment process does not appear to be one in which pure self-selection transpires, and there is plenty of room in selecting committee memberships for congressional party criteria and purposes to come into play. (2) Most House committees have memberships reasonably representative of the House as a whole in ideological or geographical terms, so most representatives "do not serve on committees that are dominated by preference outliers" (p. 79). (3) Analysis of the incidence of party voting in the House since the New Deal era of the 1930s indicates a shrinkage in party policy agendas, along with considerable stability in support among partisans for the majority (Democrats) party leaders' stands on policy issues and goals. (4) Party leaders demonstrably take party loyalty into account in making the committee assignments of freshmen representatives, and in determining the transfer of members from one committee to another. (5) That the majority party dominates committee deliberations is evidenced by patterns of bill sponsorship and dissents from committee reports on bills; majority party members sponsor most major legislation and dissent from committee reports far less frequently than minority members.

Cox and McCubbins paint a picture of congressional politics much different from the conventional view. To these authors, the congressional parties have not declined in the post-World War II years. Representatives come to Congress from a vortex of partisan electoral experiences. Intra-party ideological affinities are substantial. Members are pervasively loyal to their party leaders. The party leaders can exert considerable leverage over committees because they have "structured" the committee system itself, committee behavior tends to reflect partisan loyalties, and the party leaders tend to shape the legislative agenda. Indeed, the authors think that, in the House, "the majority party acts as a structuring coalition, stacking the deck in its own favor—both on the floor and in committee—to create a kind of 'legislative cartel' that dominates the legislative agenda" (p. 270). As in Hobbes's *Leviathan*, the governing representatives resolve collective dilemmas by establishing a central agency or authority, in the instance of the house by electing and following their party leaderships.

Why have the congressional parties become more important in the last two decades? Cox and McCubbins ask rhetorically, "Did parties, like Lazarus, rise from the dead?" (p. 277). They contend that "the parties never died . . ." but rather "that we have had a version of 'party government,' in the House at least, throughout the postwar period" (p. 277). But they also argue that, by the 1970s, the amelioration of the stalemate between northern and southern wings of the House Democratic majority—precipitated by the reduction in numbers and liberalization of Southern Democrats—permitted a series of decisions that empowered the majority leadership. Indeed, Cox and McCubbins argue that the committee reforms of the 1970s were understood by majority party leaders as a way to enhance their control of the legislative agenda.

I find this book enormously refreshing, even liberating. The authors begin the task of restoring political party politics and party leadership to their proper place in analyzing congressional life. The theoretical orientation advanced by the authors is a rational choice perspective, but the findings and implications of the analysis do not, in the end, depend upon this. Indeed, sometimes the rational choice lingo and formalization are more an interruption than illuminating. One chapter on collective dilemmas in general is very tedious, and in a few other places the argument degenerates through excessive abstraction. Beyond the authors' formalizing pretensions, the substantive argument will be vigorously debated by conventional congressional scholars who will cling to the "weak parties" or "no parties" assumptions and who can find allusions to "leviathan" and to a "legislative cartel" egregious, and by rational choice purists who may find that relaxing the ceteris paribus assumptions to which they have grown accustomed is a sop to institutional verisimilitude. But, on balance, I think this book is an important landmark on the road to a more interesting, more political, and a theoretically invigorating line of research on congressional politics.

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At Women's Expense: State Power and the Politics of Fetal Rights. By Cynthia R. Daniels. Cambridge: Harvard University Press, 1993. 183 p. $19.95.

Daniel's well-written and cogently argued book addresses the important issue of fetal rights outside the narrow context of abortion, where it is too often exclusively situated. Like Zillah Eisenstein (and Robin West), Daniels centers our focus on the pregnant body to expose the inadequacy of the liberal concept of "citizen" which is gendered male. Daniels carefully examines three legal cases to challenge the ability of classical liberalism's notions of individualism, privacy, and self-determination to defend women's rights during pregnancy.

Chapter 1, aptly entitled "Fetal Animation," traces changes in technology, rhetoric, and law on fetal rights. Each of the next three chapters spotlights one case. The 1987 case of Angela Carder erupted when hospital administrators sought and won a court order to perform a cesarian section on a woman dying of cancer over the objections of the woman, her doctor, and her family. Unlike the episode of *L.A. Law* based on the case, both mother and fetus soon died. After reviewing the emergence of the fetus as a separate patient, Daniels draws on cases that similarly resulted in a violation of women's right to bodily integrity. Daniels reveals that only when women's commitment to motherhood is unquestioned, when women align themselves with powerful male organizations (in this case doctors), and when women can "degender" the threat to bodily integrity (in this case, constructing the issue as one of patient's rights) can they prevail. Carder's "success" consisted of a circuit court decision affirming the right of a patient to decide and the development of professional guidelines.

Daniels uses the case of *United Autoworkers* v. *Johnson Controls* to show how employers use gendered assumptions about women's reproductive capabilities and appropriate work to disadvantage them in the workplace under the guise of protecting their offspring. Johnson Controls "gender-cleansed" premenopausal nonsterilized (thus "fertile") women who worked in battery manufacturing from jobs that exposed workers to lead. Daniels brilliantly demonstrates how employer's rhetoric first creates a fissure between maternal and fetal interests and then blames women for adverse reproductive outcomes—shifting the circle of causality away from fathers or the hazardous work environment. Fetal protection policies construct employers, rather than women, as the rational protectors of the fetus. The Supreme Court's unanimous decision in 1991 declaring Johnson Control's policy in violation of Title VII of the Civil Rights Act of 1964 marks a shift from resolving ostensive conflicts between work and motherhood always on the side of motherhood.

Chapter 4 ("The Politics of Vengeance") explores the 1989 case of Jennifer Johnson, the first woman in the United States to be convicted for delivering drugs to a minor by "delivering" cocaine to her newborns through the umbilical cord. Daniel uses the Johnson case to analyze the myths about the pregnant addict in public discourse and to bemoan our collective need to blame individuals for social problems. She methodically considers the medical evidence generating the hysteria over "crack babies" and the role of the media in this episode. Even more so than "fertile" women, the pregnant addict is more easily constructed as irrational. By showing that black women are 10 times more likely to be prosecuted for drug abuse during pregnancy, for example, Daniels carefully documents how class and race affect constructions of motherhood and determine when state intervention is accepted as appropriate. The analysis in this chapter is superb and merits excerpting for those unable to use the entire work.

Those who teach law and society, women and the law, women and politics, feminist political theory, public policy, or even introductory women's studies courses will cheer the publication of this book. Daniels's prose is unusually clear, readable, and accessible to those with little background knowledge. Its length also makes it a suitable text or introduction. A virtue of the book is its crossing of legal subfields: it considers cases in constitutional, criminal, family, and labor law. Daniels's work deftly illustrates the dividends of employing gender as a conceptual tool to disrupt settled legal divisions. Unlike other legal scholarship, Daniels eschews the excessive legal citations and references to other scholars that often overwhelm the uninitiated reader.

This virtue is also a limitation. Daniels adds no new information to the stories of the legal cases. Nor does she expand our understanding of legal doctrine. Her analysis is cursory about the social forces that produced these cases and the choices and strategies of the players (plaintiffs, interest groups, legislators, etc.) The book illuminates different aspects of the public policy process from those found in her previous scholarship. To regard these omissions as flaws, however, would be to miss the purpose and importance of the book. As a political theorist, Daniels employs careful analysis of contemporary cases to interrogate liberal individualism and rethink what constitutes citizenship. As a feminist theorist, she meets the challenge posted by historian Joan Scott to understand *difference* in a more sophisticated way and to unpack the gendering of social issues and concepts. She impressively meets the need to understand gender, race, and class as mutually constituting categories of oppression. In fact, the concrete illustration of this theoretically complex concept is one of the main strengths of the work.

Daniels's terse argument is unlikely to persuade those who do not share her assumptions about women's rights and reproductive choice, but that is not her aim. To those who feel discomfort when doctors perform cesarian sections over the objection of the woman, are aghast that women must be sterilized to hold a job, and look with unease on criminalizing pregnant addicts, this book powerfully analyzes each issue and brilliantly shows the connections between them. Her work is a major contribution to the emerging literature on this important topic.

University of Iowa SALLY J. KENNEY

Regulatory Politics in Transition. By Marc Allen Eisner. Baltimore: Johns Hopkins University Press, 1993. 246p. $45.00 cloth, $14.95 paper.

One of the most exciting recent developments in political science is the attention devoted to historical analysis—the *rediscovery* of history, if I may be permitted a mild exaggeration. Clearly, the dominant and, to date, the most fruitful approach within the politics and history movement is institutional development. The pioneering work of Stephen Skowronek and Theda Skocpol laid the foundation for a burgeoning interest in the theoretical connection between institutional development and the broader evolution of political systems. Eisner's new book, *Regulatory Politics in Transition,* is a contribution to this literature.

Eisner presents us with a historical–theoretical essay that seeks to make sense of shifts in regulatory policy and politics in the United States. Drawing directly on Harris and Milkis's *Politics of Regulatory Change* (1989), Eisner sees regulatory politics as evolving from one *regulatory regime* to another. The major conceptual distinction between the two works is the relative emphasis placed on ideas in the emergence of regulatory regimes. Thus, Eisner defines a *regulatory regime* as "a historically specific configuration of policies and institutions which structures the relationship between social interests, the state, and economic actors in multiple sectors of the economy," while Harris and Milkis he notes, define the concept as "a constellation of (1) new ideas justifying government control over business activity, (2) new institutions that structure regulatory politics, and (3) a new set of policies impinging on business" (p. 1). Eisner does devote considerable attention to ideas but does not accord them a primary role in explaining regulatory regime transition. Rather, the main trajectory of development runs essentially along the following path: economic change stimulates social groups' policy demands, which in turn give rise to a policy and reform agenda, out of which emerges new regulatory institutions and new policies. This process purportedly explains the emergence of four regulatory regimes in American history: the Progressive Era and the New Deal, both of which most scholars acknowledge without benefit of the regime framework, the *societal regime* of the 1970s, and the *efficiency regime,* which Eisner introduces to account for the Reagan and Bush administrations' response to economic stagnation and sharply increasing regulatory costs.

The major strength of *Regulatory Politics in Transition* is that it systematically assesses a defining element of the modern American state, namely, regulatory policy. Eisner correctly notes that the relationship between government and business provides an important window on political development. In addition, he strives to explain the unfolding drama of regulatory transition, as well as the roles played by the major institutional and organizational actors. In that enterprise, he invites us to examine critically the explanation he provides. Laudable as that enterprise is, it falls somewhat short, primarily because the framework of analysis weakens in its explanatory power as Eisner moves from earlier to later transitionary periods.

Eisner's conceptual framework is most persuasive in its application to the Progressive Era. For that period of regulatory development, he ably demonstrates how a momentous economic change (i.e., the transformation from an essentially regional and agrarian economy to a national and industrial economy dominated by large corporate organizations) evoked broad popular demands for government intervention. In addition, the connection is clearly drawn between popular agitation and the contemporary administrative doctrines, thereby explaining the character of progressive institutions and policies. Problems with this analysis begin to arise, however, as we move to the New Deal period.

The first difficulty is Eisner's designation of the Great Depression as the economic change variable in his framework of regulatory transition. While there is no denying that the Depression was an event of cataclysmic proportions, we may ask whether it is equivalent to the structural transformation of the economy described as the prime mover in the Progressive Era. An analagous transformation might have been the bifurcation of corporate ownership and control identified by Berle and Means, yet there is no real discussion of structural change. The question, therefore, is, What does the economic change variable mean? If it does not mean structural change but simply severe dislocation, we may ask why the severe depressions of the 1890s did not occasion the emergence of a regulatory regime. Further, if dislocation will suffice as a variable initiating the process of regulatory transition, how severe must the dislocation and accompanying fears of uncertainty be? The second difficulty in the analysis of the New Deal is that the role of "social group's policy demands" is assigned to "realignment" and the "formation of the New Deal coalition." Not only is there no satisfactory explanation of how social groups influenced either the 1930s realignment or the formation of the New Deal coalition, but there is very little illumination of party politics outside of a discussion of FDR's Commonwealth Club Address and the 1932 party platform, neither of which, according to Eisner, shed much light on the New Deal regime that eventually took shape. That being the case, one may well ask if his causal mechanism of economic change actuating social groups to define a reform agenda is operative? Indeed, the greatest emphasis appears to be placed on the role of the brains trust and FDR's "legendary political skill," rather than the activities of social groups.

As Eisner moves to an application of his framework in the transition to the societal regime of the 1970s, more vexing questions arise about the causal variables at work. In his schematic representation of this transition, the economic change variable is designated as the "emergence of postindustrial values" and "social unrest of the 1960s." In all fairness, Eisner attributes postindustrial values to sustained postwar growth, though this linkage is asserted, rather than explicated. Still, it is difficult to rationalize the identification of emergent values and social unrest as economic change. Perhaps, with some more analysis, it would be possible to show that postindustrial values are an intervening variable connecting sustained growth in real incomes with social unrest, which in turn stimulated social groups' demands. However, this is not the model of transition elaborated at the outset. As in the case of the New Deal, Eisner does a very credible job of relating the general story of regime transformation but does not convincingly fit that story into the framework of analysis that he has laid out.

Finally, in the instance of the new efficiency regime that Eisner claims to have discerned, we find perhaps the most strenuous contortions to make the facts fit the framework. In this case, the economic change variable is "stagflation, foreign competition, and regulatory compliance costs." Presumably, this variable operated the same way as the Great Depression in evoking dislocations and uncertainty. However, that line of reasoning raises the same questions about whether economic change means structural change à la the Progresive Era or can mean simply economic hardships. If the latter will do to initiate regime transition, we face the severity issue: Were the economic problems of the 1970s (certainly less disasterous than the 1930s) severe enough to set in motion the forces Eisner postulates as the prime movers in regulatory transition? Here even Eisner cannot identify the forces he claims regularly operate. Specifically, no social groups' demands emerge that remotely resemble the kind of reformist agitation associated with earlier regimes. A telltale sign of Eisner's theoretical dilemma may be seen if we juxtapose his introductory assertion, "Rather than being the product of an oppressive elite or of a partnership between monopolies and the state, the major regulatory initiatives of the past hundred years *originated in popular demands*" with his argument that the efficiency regime "*has its origins in elite politics* and the analytic biases established as a result of administrative change" (pp. xiv, 172, my emphasis). Thus, Eisner abandons the principal causal mechanism of his analytic framework in an effort to explain the emergence of the efficiency regime.

Ultimately the problem with this book's theoretical approach is probably that it tries to explain a recurrent phenomenon, regulatory regime change, by applying a static model across historical time periods. At each stage of regime development, the very institutional changes that Eisner describes may have substantially changed the context in which he applies the model that works so well in the Progressive Era. Like many heroic efforts at explaining long-term political development, Eisner may have fallen short of his goal; but that places him in some illustrious company. Moreover, we are indebted to him for taking on such a challenging task both because he demands that we critically reassess our own notions of modern state building and because he provides a valuable account of regulatory politics over the last century. For the latter reason, I suspect that many of us interested in the history of regulation or business–government relations will be cribbing lectures from *Regulatory Politics in Transition*.

Rutgers University, Camden — RICHARD A. HARRIS

The Shaping of American Liberalism: The Debates over Ratification, Nullification, and Slavery. By David F. Ericson. Chicago: University of Chicago Press, 1993. 238p. $40.00 cloth, $14.95 paper.

The Lincoln Persuasion: Remaking American Liberalism. By J. David Greenstone. Princeton: Princeton University Press, 1993. 312p. $24.95.

The Lincoln Persuasion is a posthumously published book that is without two of the chapters David Greenstone intended for it to have, because they were not finished at the time of his death: one on the abolitionists Lydia Maria Child and Frederick Douglass and one on Daniel Webster. It is a shame to be without Greenstone's insights on these figures, but the book as an argument is substantially complete and does not suffer from the absence of these chapters. David Ericson's book derives, in part, from Greenstone's work (his book is dedicated to Greenstone's memory), yet there are significant differences in their understanding of American liberalism.

These two books continue an argument begun by Louis Hartz in modern American political science over whether American political life is, and always has been, fundamentally liberal. Against those such as Bernard Bailyn, Gordon S. Wood, and J. G. A. Pocock, who argue that one can find a dominant or at least significant preliberal republicanism in the eighteenth century and the founding period, both David Ericson and David Greenstone maintain that the character of American politics has been consistently liberal. Yet both believe that there is merit in the criticisms made of Hartz by his critics, and each adds to these criticisms. Each tries to save Hartz's basic thesis while admitting the meritorious criticisms by arguing that within America's basically liberal tradition, there are two forms of liberalism. Hartz's critics are wrong to think that there is a significant antiliberal tradition in America but right to think that there have been important disagreements and discontinuities (albeit intraliberal ones) in American thought and practice.

It is in identifying these two forms of liberalism and their significance for understanding the Civil War and the pivotal figure of Abraham Lincoln that the two books diverge. According to Ericson's construction, the common liberal tradition understands the purpose of government to be to secure liberty and protect rights. Within this tradition, there are two distinct species of liberalism: *republicanism* and *pluralism*. The complicated distinction between these liberal species (16 propositions characterize republicanism) can be understood as presenting the two poles of a continuum. Republicanism emphasizes the importance of participation in free government and of the common good that liberalism entails, and pluralism emphasizes the importance of the private exercise of rights. Statesmen can then be placed on this continuum, and, Ericson argues, the history of American political culture can be understood as a movement from republicanism to pluralism. A concern for at least the instrumental value of civic virtue gives way to the unchecked pursuit of private ends. To show this, he examines three significant periods—those that saw the debates over ratification, nullification, and slavery. In each case, he argues that the form of the debate is determined by the relative dominance of the republican perspective (in the first two) or the pluralistic perspective (in the third), while the substance of the debate is decisively shaped by the relevant statesman's traditionalism, social realism, or nationalism.

According to Greenstone, the two species of liberalism are *humanist* and *reform* liberalism. Both kinds of liberals believe in the common liberal understanding that just government rests on the consent of the governed and has the aim of protecting individual rights and private property. Within this consensus, the humanist liberals are primarily concerned with the satisfaction of individual preferences, while the reform liberals are primarily concerned with the development of the faculties of individuals. Or (we may say), the humanist liberals are concerned with the exercise of rights, while the reformists liberals are concerned with the development of individuals who can exercise those rights well. (To illustrate, Martin Van Buren is assigned to the first camp, John Quincy Adams to the second.)

One result of these analyses is significantly differing views of Lincoln. For Ericson, Lincoln fits neatly (or almost neatly) within his scheme as a person with anachronistic republican tendencies but whose overall effect was to accelerate the inevitable triumph of pluralism. Lincoln is a significant figure (but only one of several) within the predictable history of American liberalism's movement from republicanism to pluralism. Greenstone presents a subtler and more interesting portrayal of Lincoln. He argues that Lincoln successfully reconciled reform and humanist liberal strands (unlike the reform-liberal abolitionists or the humanist-liberal Stephen Douglas). This reconciliation enabled Lincoln to arrive at a comprehensive and politically effective understanding of the slavery issue that drew on the deepest reaches of the culture, uniting the moral and religious intensity of New England Puritanism with political realism, prudence, and devotion to union. The result, argues Greenstone, was a new liberal synthesis that amounted almost to a refounding of the country. Few writers have had the appreciation for the depth of Lincoln's political understanding that Greenstone displays.

Both books contain observations and arguments from which one can learn. Yet both have a common and serious methodological flaw that unnecessarily narrows their perspective and confuses their arguments. Both Ericson and Greenstone consider the conversation in which they are participating as bound within the confines of academic political science (and only a portion of political science, at that) and refuse to consider the people they are studying as participants in this same conversation. The result, in Ericson's case, is to confuse his argument with such infelicitously denominated distinctions as the one mentioned between "republican" and "pluralistic" liberalism. *Republican* is a term with an ancient lineage stretching back to classical antiquity; *pluralism* is a twentieth-century American invention. Does it really make sense to use these terms from such different contexts at the basic antinomies within Lockean liberalism? The answer, in case anyone is in doubt, is *no*. They do not clarify but, rather, confuse the issue and trivialize what is at stake in understanding republicanism or pluralism. If Ericson took more seriously, for example, the *Federalist*'s claim to present the true understanding of republicanism, he could not use the term in the confusing way he does. Denomination of vague collections of beliefs would have to give way to a precise grappling with the issues that actually moved American statesmen. Instead of beginning with the issues raised by Louis Hartz, Ericson would have understood American liberalism better had he first feasted with Jefferson or Madison and then let Hartz come for dessert.

In Greenstone's book, one gets the impression of a naughty boy who is not supposed to talk to strangers outside of political science but who has perhaps secretly stayed up all night talking to Adams, Jefferson, and Lincoln. On the surface, he acknowledges only to having talked with Wittgenstein and the public choice theorists, and the poor reader must work awfully hard to see the choicer conversation in the depths. Greenstone would have us believe that Lincoln is all mushiness, a vague "persuasion" that cannot rival the precision of a political scientist. His account of Lincoln and his obvious admiration for him belie this impression, but Greenstone obscures his own account because he does not want to admit the company he has kept. Nevertheless, he does somehow see that the heart of the question about American liberalism is this: Does liberalism provide an adequate understanding of, and direction to, American politics, or does attentiveness to political life compel one to go outside of liberalism for this understanding and direction? And he sees that there is no better way to begin to come to grips with this question than to listen to Abraham Lincoln.

Why not admit that the person you are studying may wind up being your teacher or at least your colleague?

University of Dallas GLEN E. THUROW

The Craft of Justice: Politics and Work in Criminal Court Communities. By Roy B. Flemming, Peter F. Nardulli, and James Eisenstein. Philadelphia: University of Pennsylvania Press, 1992. 217p. $24.95.

This is the third book based on a National Institute of Justice–funded study of the criminal court systems of nine middle-sized communities spread among three states (Illinois, Michigan, and Pennsylvania). In the first two books, Roy Flemming, Peter Nardulli, and James Eisenstein developed detailed arguments about the guilty plea process (*The Tenor of Justice*) and the nature of the interrelationships among the actors in the criminal court system (*The Contours of Justice*). Both are major contributions to the study of courts and criminal justice, and I have regularly used *Contours of Justice* as a text.

I had known for years that a third book was planned, and I looked forward to it as one might anticipate the last part of a trilogy. Unfortunately, the result is more akin to a sequel to *The Contours of Justice*. This new book seeks to draw on the rich qualitative materials collected by Flemming, Nardulli, and Eisenstein in the nine communities studied, materials that had been used to some degree in *The Contours of Justice* and in several journal articles. The authors' problem was to find something to say that went beyond their previous discussions of guilty pleas and court communities.

The analytic vehicle the authors chose was a pair of metaphors: *community* (which had been used in *The Contours of Justice* and, to a lesser degree, in *The Tenor of Justice*) and *craft* (which had not been used in their previous work). The book is divided into three parts, each focusing on "the craft" of one of the core players in the court community (prosecutors, judges, or defense attorneys). The discussion covers political craft (for prosecutors and judges) and what I would label "front-room craft" (the craft of dealing with what goes on in the courtroom itself) and "backroom craft" (the craft of preparing cases, bargaining, overseeing a docket, etc.). Throughout the discussion, the authors return to the community metaphor to emphasize differences and interdependencies that mark the phenomena they describe.

The problem is that while the metaphor of community works, the metaphor of craft fails, at least as used here. The community metaphor works, at least in part, because it is based on a solid conceptual foundation: it joins the ideas of "courtroom workgroup" and "local legal culture"—both developed by political scientists (including these authors) writing about the criminal courts in the 1970s. The key idea is that there is a community of actors, who form concentric circles of *core players* (prosecutors, defenders, and judges), *peripheral players* (political parties, news media), and *outsiders* who regularly become involved (defendants, voters). These communities are marked by interdependencies, norms, and sponsoring organizations. Importantly, there is a great deal of variety among the communities; and simple generalizations about how the criminal justice system operates (plea bargaining, impact of calendaring systems, etc.) are not possible.

Why, then, does the new metaphor of craft fail? Unlike community, the idea of craft, while it is something we are familiar with in common parlance, is not something that most social scientists have considered as a research construct. I was intrigued by the prospect of seeing the idea of craft applied to the work of the actors in the criminal justice system. However, I was disappointed by the absence of any systematic development of the metaphor, and I was struck by a lack of fit between the idea of craft and a lot of what is discussed in *The Craft of Justice*, much of which seems to have more to do with functions or tasks than with craft.

Even when the authors do define craft they do so inconsistently:

> The craft of justice refers to how [the key players] pursue their individual careers, go about their tasks in the courtrooms, organize themselves for collective purposes, and interact politically in the institutional realm of the courthouse. Their craft is both practical and political; it refers to learned and adaptive knowledge that guides practitioners in getting things done within the courthouse. (p. 4)
>
> Craft is practical knowledge of how others perform their work and of the relationships involved in this work. It combines personal experiences with the lessons learned by others so that legal practitioners can organize their work and manage their relations. (p 195)

I came away from these definitions quite confused: Why is knowing how to "get along" a craft (it is something that anyone working in a social setting has to do)? Why is knowledge of how others perform their work a key component of craft? Why is craft limited to practical knowledge? (Is the preparation of a legal brief not a fundamental component of craft?) What do calendaring systems have to do with craft? Why is the relationship of reputation to craft?

I could go on here with many examples. I am not suggesting that these are unimportant or uninteresting problems, only that the craft metaphor fails to inform. Of course, there are elements in the book that do clearly relate to craft. This seems particularly true in the discussion of defense attorneys. The best chapter in the book is one entitled "Defending in the Courtroom," which relates most clearly to craft-type issues (knowing when to file motions and when not, knowing when to go with an open plea, knowing how to "judge-shop").

In summary, *The Craft of Justice* is not as significant a contribution to our understanding of criminal courts as were the previous two books that comprise the trilogy. Whether this is because the authors had really exhausted the core of their material in the previous two books or because they failed to capitalize on the theoretical potential of the craft metaphor is difficult to judge. In any case, the totality of Flemming, Eisenstein, and Nardulli's work in the nine-community study stands as a major contribution to the study of justice and politics in the lower courts.

University of Wisconsin, Madison HERBERT M. KRITZER

Declaring Independence: Jefferson, Natural Language, and the Culture of Performance. By Jay Fliegelman. Stanford: Stanford University Press, 1993. 268p. $32.50 cloth, $12.95 paper.

Starting from the premise that the Declaration of Independence was meant to be read aloud, Fliegelman takes his readers on a high-speed chase across the landscape of American popular culture in the early national period. He argues that the revolution challenged Americans to "imagine social relations on terms other than familial" (p. 191). As the new nation embarked upon "the postcolonial project of deriving an indigenous identity from the creative appropriation of Old World materials as well as from their rejection" (p. 187), oratory became a crucial site for the reconceptualization of political authority and, ultimately, for the production of a distinctively American sensibility.

Before oratory could assume such a culturally central position, it, too, had to undergo a revolution. Thus, Fliegelman reconstructs the rise of an elocutionary ideal that was essentially performative and whose aim was to create a sympathetic identification between orator and audience. This new rhetoric treated the body as an instrument and focused on "sounds, tones, and facial expressions," rather than words and arguments (p. 48). Because the orator's task was not so much to express feelings as to call them forth in others, this ideal set in motion (but also contained) a series of conflicts—between the natural and the theatrical, self-control and self-revelation, particularity and representativeness, and imitation and originality (pp. 64, 89, 115).

Fliegelman is "concerned with rethinking and expanding the kinds of 'facts' that are traditionally judged to be relevant to understanding a major historical document"; "In the case of the Declaration," he argues, "those facts include . . . the structure of a fugue, the particular design of a chair, the way Jefferson wrote the letter *s*, accusations that Milton's *Paradise Lost* was plagiarized, and the eighteenth-century assumption that blacks are unable to blush" (p. 4). If this book does nothing more than alert students of political rhetoric to the wide range of cultural referents that might be brought to bear in their interpretations of texts, its author will have performed a valuable service. For some readers, Fliegelman's exuberance, erudition, and eye for the telling image will prove compelling. But those who approach the book more analytically—seeking either a fresh interpretation of the Declaration or a model for cultural history—are likely to be disappointed. Ultimately, *Declaring Independence* enacts the ideal it describes: its argument seems less important than the performance itself.

The book is full of interesting bits of information. We learn, for example, that the term *responsibility* made its first appearance in print in the *Federalist* and that the committee charged with drafting the Declaration of Independence also proposed a national seal whose iconography featured the Red Sea overwhelming a crowned Pharaoh after Moses and his people had made their way safely to the opposite shore (pp. 143, 160–64). But while *Declaring Independence* offers a wealth of new images and sources that should enrich our understanding of the early national period, it does not provide a very persuasive interpretation of the texts with which we are already familiar. For example, *Declaring Independence* seems unable to account for the one fact that political scientists are likely to know about oratory in the early republic, namely, that the federalists condemned demagoguery and promulgated a constitution designed to ensure that reason, rather than passion, governed political decision making. Fliegelman mentions "the Tory attack on Whig oratory as demagogical social climbing and inflammatory propaganda"; but his suggestion that this attack "failed to recognize that such propaganda rested . . . on what may be seen as a new emotive theory of representation . . . consistent with the objectives of the new oratory" does not explain why (or what difference it makes that) once the revolution had been won, the federalists adopted the Tory position (p. 76).

The weaknesses in Fliegelman's argument stem from his apparent unwillingness to grapple with questions of cultural production. Lurking beneath the postmodern vocabulary and concerns of *Declaring Independence* is a rather old-fashioned brand of intellectual history. Culture seems to consist of a series of texts that share the same language and concerns. Though forewarned that "this study proceeds by associative progression rather than by the isolation of different discourses" (p. 4), the reader hardly expects to encounter a work in which the only principle of association seems to be resemblance. Fond of the dialectic and of dilemmas, of ambiguity and anxiety, Fliegelman sees tension everywhere, but, of course, the same tensions keep recurring.

Curiously, this emphasis on "internal conflict" undercuts the possibility of any genuine contestation. Symptomatic of this problem is Fliegelman's failure to distinguish between federalists and antifederalists. When, for example, Fliegelman claims that the American ideal of representation called for the construction of a legislature that would be a mirror image of the people, he quotes George Mason's critique of the Constitution of 1787 as if it were a description of the theory that governed the Constitution's design (p. 43). Maybe this is just poor research (Fliegelman cites Morgan citing Mason), but it is also the kind of mistake that is a likely artifact of a method that selects similar passages from very different kinds of texts and then juxtaposes them without analyzing the rhetorical or institutional contexts in which they occur. Such an approach systematically suppresses the possibility that the same culture (or maybe the same nation) can contain radically different conceptions of its most fundamental concepts.

Fliegelman's treatment of contemporary scholarship poses a similar problem: nothing challenges his thesis, because everything can be incorporated into it. In *Letters of the Republic* (1990), Michael Warner made a quite compelling argument that seems diametrically opposed to the thesis advanced in *Declaring Independence*. Warner contends that republican ideology was embedded in a print culture that made impersonality a norm of public political discourse. Though praising the book in a note, Fliegelman dismisses its central claim in a single paragraph, stating that Warner has overlooked the fact that print culture operated alongside the oratorical conventions Fliegelman describes. "Indeed," Fliegelman argues, "what energizes a great deal of the polemical prose of the period is the dialectical relation between the authority of impersonality . . . and the authority of sincerity" (p. 129).

This book would be considerably more powerful if Fliegelman were as sensitive to the differences between discourses as he is to the similarities across them. Both Richard Brown's *Knowledge Is Power* (1989) and Cathy Davidson's *Revolution and the Word* (1986) suggest that there was more than one way of constituting authority in the early republic and that each kind of authority had its own audience, logic, and institutions. Brown shows how, in the course of the revolution, lawyers came to eclipse ministers as the official spokesmen for their communities. Davidson discusses how both political leaders and clergy thought that the rise of the novel threatened to undermine their authority. At stake in acknowledging the contestation between discourses is not simply a preference for conflict over consensus but the difference between a static and a dynamic conception of culture. When culture is conceived of as something that leaves the same imprint on all of the artifacts of a particular era, the only ways to conceptualize change are as revolution or degeneracy. Fliegelman opens on the first note and closes on the second. By the end of his narrative, the "quest to find a voice and visual self-presentation that are true to nature, society, nation, and self has become . . . little more than an existential confidence game" and the bottom floor of the house in which Jefferson drafted the Declaration of Independence has become a clothing store whose handbills proclaim every American's right to "Life, Liberty and Genteel Garments" (pp. 196, 200).

Johns Hopkins University SUZETTE HEMBERGER

Facing the Bureaucracy: Living and Dying in a Public Agency. By Gerald Garvey. San Francisco: Jossey-Bass, 1993. 252 p. $23.95.

Inside NASA: High Technology and Organizational Change in the U.S. Space Program. By Howard E. McCurdy. Baltimore: Johns Hopkins University Press, 1993. 215p. $32.95.

These two very good books use the theme of the organizational life cycle as a point of departure for the analysis of two organizations, the Federal Energy Regulatory Commission (FERC) and the National Aeronautics and Space Agency (NASA). Each story is anchored in a theoretical theme: bureaucracies lose resilience as they age; controls from the top are imposed in response to the loss of resilience; the new controls cause new transactions costs in the work of the organization and impair effectiveness; aging is only one reason for the loss of resilience; reform efforts to reduce controls and yet ensure accountability are difficult and uncertain in their effectiveness.

Garvey tells the story of the birth, life, and death of a project in the FERC to computerize and automate the system of rate making for the natural gas industry. He was a consultant to the project and writes from a participant observer stance. He posits that the "old theory" of public administration invented by the Progressives, which sees bureaucracy as efficient and politically accountable, is unable to explain the discoveries of the "new theory" about adverse selection, moral hazard, and the use and misuse of discretion. The chairman of the FERC was concerned about the even-handedness of a decentralized regulatory process that rested on bargaining between rate makers and private firms. Garvey was appointed a consultant through a bureaucratic end run that was later challenged. The process of reform worked initially, because an experienced administrator appointed a task force of computer consultants and key rate makers, which, over time, developed a new system to simplify the work of rate setting and make it more uniform yet permit discretion on the part of individual regulators. A consensus on the new standards was reached, but the achieved plan was only a half-way house toward a comprehensive system. Then an internal investigation of the project arose out of a feud among top agency officials, the method of Garvey's appointment was challenged, the appointment was invalidated, and the computerization project was killed by agency administrators worried about a more extensive inquiry.

Garvey draws practical lessons from the story. Organizational entrepreneurs can initiate directed changes against the natural changes that institutional aging brings. The key to success is public managers with political sense who understand effective strategies of organizational reform guided by *prudence*. Reform of this kind must take advantage of the unfulfilled personal aspirations of civil servants if it is to work; that is, the slack found in such aspiration is the best basis for helping people to do their jobs better through reform. He finds the "old theory" of public administration a better guide to action here than the "new theory," which sees bureaucracy as permeated by "self-regarding" motives. Old theorists understand the importance of organizational culture, to which new theorists are blind. Garvey sees the naïveté of both the old and new theories and seeks to combine the idealism of the old and the tough-mindedness of the new in a thesis about prudential leadership.

Garvey does a good job of making a case study theoretically pertinent. The story is a bit slow, perhaps because it is not inherently interesting, and the participant-observor stance is one person's view of events. The ultimate normative issues are not resolved, but the book will contribute to the development of theory about organizational renewal.

McCurdy describes the origins of NASA out of agencies-charged with research-and-development for aeronautics in the interwar period. These agencies and the founding NASA created an organizational culture that gave highest value to in-house research-and-development, testing of products through hands-on engineering, and tolerance of failure through high-risk strategies—in short, a paradise for innovative engineers. The combination of youthful talent, a supportive political environment, and the historical opportunity to invent exciting technology fostered an agency that valued inquiry, open communication, attention to detail, honesty, idealism, and romance.

But after the excitement of putting men on the moon, public interest waned, political support declined, and the NASA became an operating agency, offering less challenge to its engineers and more opportunities for outside contractors. Centralized bureaucracy became stronger, and the culture of technical inventiveness eroded. Congressional oversight increased, White House control was heightened, the regulatory ethos of government grew oppressive, and bureaucratization burgeoned. Administration won out over technical invention, the dependence upon contractors increased, and the capacity to oversee them declined. The decline of in-house testing of products made the punishment of failure even worse because of limited public sympathy. The communications failures seen in the *Challenger* explosion and the Hubble Space Telescope mishap were manifestations of an organization that had lost the "taut communication" of the old NASA.

McCurdy concludes that high-performance bureaucratic cultures are inherently unstable. Once a governmental agency matures, the political environment that had supported its dynamism has disappeared. The most successful science and technology programs have been able to circumvent the surveillance that controls most bureaucracies. The Manhattan Project, Tennessee Valley Authority, (TVA) and other such agencies cannot be sustained in the normal bureaucratic mainstream. And once on the decline, it is very difficult to put new wine in old bottles.

McCurdy's research effort was extraordinarily comprehensive, relying greatly on interviews and surveys. Had he written a longer book, we would have a richer sense of the changing bureaucratic culture. Many of his descriptions of professional and subagency perspectives are redundant. Above all, I wish that he had given us a portrait of successive NASA leaders so that we might assess their contributions to the growth and decline of the organizational culture. As he presents it the NASA is virtually leaderless. But again, this book is a strong contribution to the theory that will emerge in time.

The two books together pose a number of important theoretical insights. First, the aging of an institution is important not in itself but for its actions and reactions. The completion of the initial missions stated by the founding charter, a changing political environment, and new generations of leaders all combine to explain the cycle of rigidity and control. Life-cycle theory is thus useful for students of bureaucracy not as a teleology but as a source of insights of opportunities and constraints on action at different times of an organization's history.

Second, politics and the pressures of politicians make it hard to keep agencies lean and innovative. Politicians will pile on encumbrances for reasons of their own often having little to do with the work of the agency. This seems to be especially true after an agency has passed its peak creativity. Incorporation into the bureaucratic mainstream effectively kills its spirit. But perhaps that is what should happen if the heroic mission has been completed.

Third, agency inventiveness and autonomy go together. The insulation from politics is the key to success. Political accountability engenders bureaucratic timidity; but again, perhaps this is appropriate if the initial agency autonomy was empowered by popular politics, as was the case with the NASA.

The systematic comparison of organizational stories like these within a life-cycle framework might permit the development of rich theory about diverging patterns in institutional history. Organizations can become stagnant like the NASA, discredit themselves by trying to stay heroic like the TVA, engage in prudent reform like the FERC, or engage in widespread organizational renewal like the General Accounting Office in recent years. Such a theory would permit us to assess the relative importance of leadership, organizational culture, and changing environments in understanding which of these paths is taken.

We also do not have sufficient prescriptions for combining bureaucratic resilience with political accountability. The two brands of "institutionalism" in political science carry different messages: the one relies on culture and norms and the other looks to mechanisms powered by self-interest. Is there a third theory that might incorporate the partial insights of the other two?

Vanderbilt University ERWIN C. HARGROVE

Latino Voices: Mexican, Puerto Rican, and Cuban Perspectives on American Politics. By Rodolfo O. de la Garza, Louis DeSipio, F. Chris Garcia, John Garcia, and Angelo Falcon. Boulder: Westview, 1992. 232p. $29.95 paper.

LULAC: The Evolution of a Mexican American Political Organization. By Benjamin Marquez. Austin: University of Texas Press, 1993. 141p. $25.00 cloth, $11.95 paper.

The past few years have seen a steady, if modest, increase in the interest in, and research on, Latinos in the U.S. political system. Not only has research increased, it has also changed somewhat in tenor and direction and has encompassed a broader range of questions and methods. Some of the earlier research on Latino politics might have been viewed, or even dismissed, in certain scholarly circles as being descriptive, impressionistic, advocacy-oriented, or otherwise out of the mainstream of political science research. The questionable accuracy of such characterizations aside, the two books under review here can hardly be criticized on those grounds. Both are quite conventional in their concerns and approaches and are certainly rigorous, serious scholarly undertakings. And both volumes represent "firsts." *Latino Voices* summarizes findings from the first large national survey of Latinos, *political* attitudes and beliefs. *LULAC* is the first book-length study to chronicle the rise and development of the League of United Latin American Citizens, a major Latino interest group, and the first work to apply Olson's "incentive theory" to such a group. Moreover, both books provide important—and sometimes surprising—insights about Latino mass attitudes, behavior, orientations, and interest groups.

These two books, in themselves and in what they promise for the future, are likely to be seen as key pieces in the puzzle of research on Latino politics. At the same time, their methods, findings, and conclusions evidence the complexity and ambiguity inherent in seeking to understand Latino politics. The similarities noted, the two books are different, partly due to substantive interest and partly due to methods. *Latino Voices* is a broad-brush treatment with a contemporary, survey data focus; the Marquez book is an in-depth, historically oriented, archivally based study. The former is consciously "pan ethnic"; the latter addresses (only) a Mexican-American organization.

Latino Voices provides an overview of findings from the Latino National Political Survey (LNPS), a survey of over 2,800 Latinos and 456 non-Hispanic whites (Anglos) conducted in 1989–90. The LNPS was modeled after—and, in fact, borrows many questions from—other major political surveys, such as the National Election Studies and NORC. Following an introductory chapter outlining the study's foci and survey methodology are eight substantive chapters with 172 tables on the Latino citizen population and one chapter with 86 tables on the noncitizen Latino population (One appendix includes the entire questionnaire.)

The goals of this volume are, as the authors explain, "modest: to provide basic information about the political values, attitudes, and behaviors of the Mexican-, Puerto Rican-, and Cuban-origin populations in the United States" (p. 1). As basic as those goals may appear, they are important in that previous general political surveys have included too few Latinos to provide reliable information. Moreover, the broad array of questions posed and the delineation of responses by specific nationality groups (including Anglos) produce a rich survey. Because the goals of this work are modest, however, the authors provide relatively brief narrative to highlight evidence in the many tables. Papers authored collectively and individually by the team of scholars involved in this research have already supplied more detailed assessments and "future volumes will thoroughly analyze" the LNPS's results (pp. 1, 6).

What are some of the more notable findings? Apart from the finding that "these [Latino] groups defy simple categorization" politically (p. 15), I suspect that readers' reactions to the data will depend on their expectations and previous knowledge of Latino politics. For several forms of nonelectoral political participation (e.g., attending rallies, volunteering for political parties, attending school board meetings), Latinos are about as likely to participate as Anglos (p. 120). This, too, is in the absence of controls for socioeconomic background; appropriately adjusted, Latino participation in these areas might equal or exceed that of Anglos. Second, the self-professed political ideology of Latinos, "regardless of national origin, was moderate to conservative" (p. 83).

Recognizing the summary nature of this work, several criticisms can be noted, nonetheless. First, while a vast array of data are presented, the tables provide only percentages. Presentation of simple summary statistics (means, medians, standard deviations) would have aided tremendously in making sense of the large and complex body of evidence. Second, many tables provide useful comparisons between the Latino groups and Anglos. However, it is not always clear why Latino-versus-Anglo comparisons are presented in some instances but not others. In some circumstances, the logic of the question indicates why such comparisons would make little sense; in other instances, that is not the case.

In LULAC, Marquez deftly portrays the history, goals, thinking, and struggles of LULAC, using archival data (group newsletters and members' personal correspondence) and interviews with group members as primary sources. Incentive theory, as developed by Mancur Olson and others, is employed to frame—and thus make sense of—the group's rise and change. Created in the late 1920s in Texas, LULAC began with "purposive and expressive" goals aimed at achieving equal opportunity—goals that ostensibly were often accomplished through litigation in state courts. LULAC underwent a second burst of "mobilization" after World War II but has been changing and, over time, has come to face a "politics of survival."

The book's "central thesis" is that LULAC's longevity has been due to "the organization's ability to effectively adjust its incentive structure during the late 1960s and early 1970s. This change in the incentive structure transformed the organization from an activist civil rights group to a staff- or elite-dominated group that would devote much of its energies to continuity and survival" (p. 7). That survival has come with significant costs to LULAC's role as a serious political actor; heavy reliance on outside funding, including governmental and foundation support, has rendered it politically irrelevant.

Despite its absorbing and systematic discussion, various questions might be raised about this study. Marquez seems to assume (perhaps too readily) that because LULAC developed along a path consistent with what incentive theory would predict, the theory is therefore (fully) supported. It may be that LULAC developed as that theory *generally* predicts, although there are some "deviations" from the predicted pattern noted in the book. More significantly, LULAC's development might be equally well understood with other theories or explanations, but incentive theory is the only explanation considered. I should like to have seen clearer, more definitive evidence to corroborate the accomplishments attributed to LULAC; as it stands, those accomplishments are more asserted than solidly supported.

Finally, a more extended treatment of the theoretical implications of this study would have been helpful. These concerns are hinted at (but only hinted at) in an all-too-brief (six-page) concluding chapter. I should like to have had Marquez's thoughts on various questions: What does the present state of LULAC, as well as that of other mainstream Mexican American interest groups, mean for Mexican American politics? For (broader) Latino politics? For interest group politics? How critical is the survival crisis facing LULAC in the bigger picture of Mexican-American/Latino politics, given the disadvantaged political and social status of the Latino population? What might all this mean for the contemporary state and future development of U.S. liberal/pluralist democracy?

These and related questions should be among the central ones shaping the Latino politics research agenda in the near future. The work of Marquez, as manifested in the present work and as it will (hopefully) be broadened and extended can offer one useful perspective on these questions. The LNPS has initiated another approach to these issues and promises to speak fruitfully to these concerns from another angle. Diverse approaches are not only desirable but necessary, given the complex and ambiguous nature of Latino politics that these two books and most research on Latino politics demonstrate. What is most important to recognize is that larger theoretical and normative questions should guide the inquiry and that approaches are secondary.

University of Colorado, Boulder Rodney E. Hero

The Hollow Core: Private Interests in National Policy Making. By John P. Heinz, Edward O. Laumann, Robert L. Nelson, and Robert H. Salisbury. Cambridge: Harvard University Press, 1993. 450p. $39.95.

It has been said that the study of pressure politics in America is characterized by a surfeit of theory and a paucity of data. In this important volume, a distinguished set of coauthors who hail from the relevant disciplines of law, sociology, and political science use what is unambiguously the richest and most comprehensive body of systematic data about *Washington representatives* (advocates for organized interests in national politics) to remedy that circumstance.

The remarkable data base underlying this inquiry consists of hundreds of interviews with Washington representatives, individuals in the organizations that hire Washington representatives, and elected and appointed government officials. The samples represent something of a "stratified snowball." The authors plumbed several sources to compile lists of organizations and individuals involved in four policy domains: agriculture, energy, health, and labor. By selecting respondents from these lists with probabilities proportional to the number of mentions, they generated, instead of a random sample, a sample that overrepresents active and influential individuals and organizations. A major portion of the book is devoted to establishing who these Washington representatives are—their backgrounds and careers, the organizations they work for, and the breadth of their policy concerns.

The heart of the book (and the section that explains its cryptic title) consists of a series of smallest-space analyses that carefully chart who knows, talks to, and fights with whom in each of the policy domains. It is here that the research strategy of concentrating on only a limited number of policy arenas pays off, for the detailed examination of the patterns of relationship would have been impossible with the more scattershot set of organizations inevitably captured when all policy domains are sampled. The communications networks and the structures of conflict and cooperation vary with the policy domain. However, in no domain do the authors find a small, cohesive elite playing either a dominating or mediating role at the center. Rather than a wheel with a hub and spokes, the networks of communication among influentials resemble a soap bubble, with an empty center and a thin surface—hence, the "hollow core." In short, the authors have developed a powerful methodology that takes us beyond the obsolete notion of the iron triangle and lends precision to Heclo's notion of the porous but diffuse policy network. This methodology could be applied to policy domains ranging from civil rights to foreign policy to taxation (domains presumably characterized by quite different patterns from those examined here) to provide a detailed map of the configuration of organized interests in national politics.

In the final part of the analysis, the authors confront, in a systematic way, the knottiest problem facing analysts of pressure politics, assessing whether and under what circumstances organized interests influence policy outcomes. Because it is more difficult to sample controversies than organizations, recent survey-based studies have told us much more about which organizations are active in national politics, what they do, and whom they represent than whether they have clout. In each policy domain, the authors located 20 major policy controversies and asked respondents several questions about each controversy, including how successful the organization was in realizing its goals. With admirable candor, the authors admit that their multivariate analysis (using such intuitively relevant variables as the number of years the respondent spent in government or the number of professionals employed by the organization) predicts surprisingly little about organizational success in achieving its policy objectives. From this they conclude that the hallmark of policy controversy in national politics is the uncertainty of the outcome.

The authors may give up too soon. They are aware of some of the potential pitfalls: a better measure of success might have incorporated the results of analysis of decision making in addition to the self-assessments of success by respondents. Other variables (esp. more and better measures of, e.g., organizational budget and the size of its PAC) might have been introduced into the equations. More importantly, however, the authors take insufficient account of the problems intrinsic to their methodology and the extent to which their original sample design exacerbates those problems. We must recall that the organizations examined have undergone several processes of selection, including the authors decision to oversample the active and influential. Thus, the authors' real finding is that when active, powerful organizations get to the table to settle a relatively important issue, the variables that might have been expected to make a difference seem not to matter. Had the sample included not just the big fish but the little fish whose efforts in Washington go unnoticed, the minnows that cannot even establish a presence in Washington, and the latent interests that never even hatch into organizations, then the authors might have found many factors that affect who wins and loses in Washington. This suggests, by the way, that students of pressure politics must not neglect two objects of inquiry that the authors of this study know they cannot pursue: how issues are placed onto (or kept off of) the policy agenda and what sort of organizations come into existence and are able to establish a substantial presence in national politics.

That the index to this volume contains more references to Talcott Parsons than to David Truman says a great deal about its place in the substantial literature on organized interests: this collaborative study owes more to sociology than to political science. This is manifest in superficial ways. For example, the authors display none of the relish—seemingly universal among political scientists who study organized interests in Washington—for colorful details and illustrative stories about particular organizations and particular disputes. Furthermore, many of the empirical findings in this relentlessly data-driven book must come as no surprise to readers familiar with the Washington scene (e.g., that Washington representatives talk to their friends and not to their enemies or that what they know counts for more than whom they know). However, it would be unjust to criticize the authors for using an expensive study and a sophisticated statistical apparatus to tell us what we already knew. That systematic data often *dis*confirm what we know from anecdotes or personal experience is especially important in a realm as rife with misleading stereotypes as Washington pressure politics.

The sociological roots of this study are evident in fundamental ways, as well. Its pathbreaking contribution—the development and application of techniques to permit the mapping of the networks of communication and patterns of conflict and cooperation in the realm of pressure politics—derives its inspiration from sociology. In contrast, several topics typically treated by political scientists do not figure importantly: we hear little, for example, about the constituencies of organizations, the constraints they impose and their role as a resource in confronting policymakers; about elections and the place of political action committees in organizational strategies; about the substance of what has become a series of marketlike exchanges between Washington representatives and public officials, each of whom controls important kinds of political capital. Finally, although the authors refer in passing to the abundant theories generated by political scientists to understand pressure politics, their study does not pay consistent attention to a series of questions about which students of democracy have been known to obsess: Who governs, how, and on whose behalf?

Boston College KAY LEHMAN SCHLOZMAN

Redefining the First Freedom: The Supreme Court and the Consolidation of State Power, 1980–1990. By Gregg Ivers. New Brunswick: Transaction Publishers, 1992. 248p. $32.95.

Religious deviance is one of the leading rationales human beings have given for depriving each other of fundamental rights. For worshipping the wrong deity or rock—or for worshipping the right deity or rock in the wrong way—people have been killed, tortured, and imprisoned. Religious minorities who avoid these fates are often treated as second class citizens in their homeland. State laws privilege dominant faith communities by teaching religious doctrine in schools, placing religious symbols on private property, using tax revenue to pay sectarian expenses and refusing to make exceptions when seemingly neutral secular policies place severe and unnecessary burdens on politically powerless religious communities. Given the numerous obstacles religious minorities face in many parts of the world, the recognition of the right to freedom of religion remains a precious and fragile achievement.

Ivers believes that this precious freedom is presently under assault in the United States. His *Redefining the First Freedom* correctly notes that "church-state separation and religious freedom are not privileged occupants in the contemporary [Supreme] Court's constellation of constitutional values" (p. 2). Conservative politicians and justices, Ivers insists, have already "emasculated the law of religious free exercise" and are a vote short of "render[ing] the establishment clause toothless" (p. 2). Unless new appointments dramatically change the recent direction of judicial policymaking, Ivers fears we may again return to a state where "majoritarian religious values" dominate the "civic culture," "exclud[ing] the needs and interests of religious minorities" (pp. 2–3).

Redefining the First Freedom clearly demonstrates that the Supreme Court during the 1980s narrowed the scope of both the establishment and free exercise clauses of the Bill of Rights. In cases concerning public aid to parochial schools, religious access to public schools and religious symbols on private property, a court remade by Ronald Reagan and George Bush was consistently supportive of popular majorities that wished to infuse public life with more religious values. The court's "highly deferential posture toward majoritarian decision making" was particularly egregious in *Bowen v. Kendrick* (1988) when the justices sustained provisions of the Adolescent Family Life Act (p. 84). That unfortunate measure offered direct federal grants to religious groups that study and respond to problems of teenage sexuality. The only serious flaw in Ivers doctrine analysis is his tendency to claim that decisions that permit government to "accommodate" religious practices require government to do so. Thus, although the Court has found the Equal Access Act constitutional, the justices have not argued that in absence of such legislation local communities would be constitutionally obligated to permit religious organizations to use school buildings for meetings.

Although Supreme Court policymaking is the primary focus of *Redefining the First Freedom*, that work makes several important observations about lower federal courts and interest groups. The Court's tendency to avoid clear statements of constitutional principle in many First Amendment cases, Ivers documents, has left lower federal courts with little guidance on such matters as how many secular symbols it takes to make constitutional a state-sponsored crèche in a public place. Ivers's examination of religious groups' involvement in recent cases refutes the too common claim that persons who oppose school prayer or aid to parochial schools are somehow hostile to religion. As he clearly shows, many religious organizations, Jewish and liberal protestant groups in particular, have fought hard over the past 20 years to maintain against political and judicial efforts to lower the wall of separation between church and state (pp. 7, 107).

Indeed, significant religious opposition to the recent direction of judicial policymaking partially undermines Ivers's claim that the justices have tolerated the infusion of majoritarian religious values into the civic culture. The mainstream Protestant ministries represented by the National Counsel of Churches have historically wielded far more political influence than the evangelical groups that *Redefining the First Freedom* sees as the Court's most influential supporters. Rather than understanding the Moral Majority as representing popular majorities, a more sophisticated treatment of interest group politics might understand that coalition as a large faction that supported Republican economic policies during the 1980s in return for some support of its religious agenda.

Ivers does not adequately analyze the more general constitutional principles that might explain why the Rehnquist Court has given more sanction to religious expression in public life. For the most part, he explains recent judicial practice as part of the reflexive judicial habit of supporting the government against claims of individual right (pp. 139–40, 173). The same Rehnquist Court justices who have done the most to limit the scope of the establishment and free exercise clauses, however, have also limited state power to pass affirmative action programs, restrict campaign spending, and take property without compensation. Hence, one might want to know why conservative justices are so eager to limit the scope of some constitutional freedoms while expanding the scope of others. Ivers might also have speculated why Justice Scalia's libertarian strand, which is so evident in flag burning and drug testing cases, suddenly deserts him when he considers the free exercise clause.

Although often thoughtful and challenging, *Redefining the First Freedom* has the feel of an extended ACLU fundraising letter. With rare exception, opinions that deny establishment clause claims are described as "blind," "indefensible and unprincipled," "illogical," "analytically bogus," "reactionary," "crabbed and misconceived," "scandalous" and "utterly indefensible" (pp. 71, 105, 107, 108, 117, 134, 148, 153). Moreover, each defeat is described as "disastrous" or "tragic," as if allowing a crèche to be placed on the town green or a moment of silence before class is the first step in a process that will eventually bring back the Inquisition (pp. 134, 173). Ivers's concern that if the Supreme Court permits "religious devotionals during ceremonial occasions, . . . will the establishment clause still have a role in securing religious freedom for . . . anyone at all" lacks any historical perspective (p. 53). The First Amendment may forbid any governmental act that directly helps a particular religion or religion in general, but Ivers is wrong when he claims that adoption of Justice Kennedy's coercion standard "would gut the core of the Establishment Clause" (p. 114). As religious dissenters in the former Soviet Union and Iraq will attest, a non-coercion requirement is the heart of religious freedom. Judicial decisions permitting too much accommodation of religion may violate the First Amendment, but religious freedom in the United States will not disappear if communities are allowed to place cheap crèches on public squares during the holiday season.

University of Maryland, College Park MARK A. GRABER

Sharing Power: Public Governance and Private Markets. By Donald F. Kettl. Washington: Brookings Institution, 1993. 232p. $22.95.

Sharing Power, by Donald F. Kettl, stands in a lengthening line of recent works on the relationship of public governments to private markets in the provision, production, and delivery of traditional public services. Kettl's work is not, however, a primer on how to share power between the two. Through a description and analysis of five "case studies." Kettl identifies some of the fundamental forces in government and in the market that make the sharing of power complex and difficult, if not impossible. Specifically, he challenges the current and popular rhetoric that declares that greater reliance on the market—what he calls the *competition prescription*—is the solution to many public problems. While he categorically states that his work *in neither an attack on nor* *a defense of the competition prescription* (p. 6), he argues that market and government imperfections create uncertainties that may cause the competition prescription merly to shift problems or to create more problems than it resolves. He further argues that government has a diminished capacity to manage shared power with the market. *Sharing Power* thus provides a cautionary note to the too easy acceptance of public governance' relying on the market and attempts to identify what is required of government and, indirectly, of the market in order for shared power to work.

As Kettl correctly notes in the first chapter, the sharing of power between public governments and the private market in the United States is not a new phenomenon. However, the historical record documents an ebb and flow in our tendency to place our hopes and faith in one or the other. Accordingly, we adopt a consensus or reliance, or (in the terms of Yehezkel Dror's *Public Policymaking Reexamined*, 1968) a *metapolicy*, that is, a policy about policies that shape reliance. In Kettl's terms, we change principal agents. Before the Great Depression our metapolicy of shared power was one of reliance on the market. Following the Depression and World War II and for the subsequent three decades, the metapolicy of the United States was one of substantial reliance on the public sector for the addressing and solving of many public problems. In the 1980s, in the United States and around the world, the perceived overreliance on, and cost of, public governance initiated the current era of heightened interest in market solutions to public problems. Government became the problem, not the solution.

In the United States, the current shared-power movement has been fueled by the convergence of three major and often ideologically incompatible forces and has taken multiple forms. The three major forces are (1) the generally antigovernment position touted by the Heritage Foundation, public choice economics, and even the 1988 *Report of the President's Commission on Privatization;* (2) government reformers seeking a better way to "do government"; and (3) the pragmatist attempt to address the problem of how to balance the budget in the context of great opposition to higher taxes. The forms of shared power range from contracts, grants, and vouchers to tax expenditures and denationalization. The labels include *privatization, public–private partnerships, third-party government*, and Kettl's *government by proxy* (see his 1988 book of that title), among others.

Kettl enters this complex arena to examine what this growing reliance on the market means in terms of how well public governments can manage partnerships in which they share power with the market. He concludes that government has not managed shared power very well and notes the irony in the many cases of shared power that have resulted in waste, fraud, abuse, and criminal activity on the part of the private-market participants. Kettl makes the case that shared power does not so much reduce the size of government as change the role of government from a deliverer of services to a manager of private-market service delivers. Based on the case studies, he concludes that the competition prescription fails because the overall problems of government are due to uncertainties and imperfections in government and in the market, not to lack of competition. He further concludes that even where shared power can work, government is not equipped to play the required shared-power management role. In the final chapters of this work, Kettl offers nine propositions that define the uncertainties in the relationship of government as buyer and market as seller and attempts to provide a prescription for how government can become a smart buyer of private-market goods and services and how government can become an effective manager.

Kettl is not the first to recognize the complex managing and governing relationships between public governments and private markets. Charles E. Lindblom's *Politics and Markets* (1977) argues that the market holds government prisoner and is thus an unequal partner. The National Academy of Public Administration's *Privatization: The Challenge to Public Management* (1989) concluded that privatization changes the practice of government and called for training and education of public administrators to manage shared power. And Heilman and Johnson's *Politics and Economics of Privatization* (1992) documented the complex relationships that exist in water-quality privatization and concluded that successful privatization would depend on the quality of the governmental and private-market contractual and management relationships. However, the study concluded that in no case should government relinquish the provision of an essential public service and a matter of governmental sovereignty (as Kettl notes). What government *can* relinquish or share is the production and delivery of a service. It is in these functions that a smart buyer and a capable manager become critical.

While Kettl more than once professes neutrality on the question of the inherent soundness of the competition prescription, *Sharing Power* is, in fact, a broadscale challenge to indiscriminate reliance of government on the market. The reader might wish for more help in sorting out the options. The organizing concept of shared power is not well defined, and the varying forms (e.g., contracting, privatization, partnerships) are used interchangeably. They are not interchangeable. Some forms challenge the sovereignty of the state, while others do not. One can be neutral on some forms and not on others. Second, Kettl's prescriptions are both too much and too little. They are too little, in that we know that effective government managers must be hired, trained, and rewarded. I would add that before hiring, they must be educated in public administration programs that teach democratic theory and public–private policy and administration. The prescriptions are too much, in that they identify the key elements needed to make shared power work—namely, to lower the rhetoric and implement the prescriptions through strong leadership—without providing a clue as to how to achieve either of these. I suspect, in fact, that Kettl's work might well raise, rather than lower, the rhetoric.

Nevertheless, *Sharing Power* addresses the important issues of shared power between public governments and private markets. At its core, this is a book about democratic theory—about what governments and markets can and cannot do. Kettl recognizes the fundamental differences between the dynamic of government and the dynamic of the market and explores what is required in order for them to share power. Anyone standing in the stream of the ongoing movement toward greater government reliance on the market (and that includes citizen, practitioner, and academic) would do well to read *Sharing Power*. Kettl sets the correct and relevant agenda.

Auburn University GERALD W. JOHNSON

The Market Experience. By Robert E. Lane. New York: Cambridge University Press, 1991. 630p. $65.00 cloth, $24.95 paper.

Robert Lane was "blindsided in the 1960s by the counterculture" and spent the next 25 years coming to terms with it (p. vii). As a result, he has produced a book that could do more to revolutionize Western civilization than anything produced by the counterculturalists. He argues not simply that markets are bad or socialism good but that all of our standard criteria for judging the market are wrong. We should not evaluate the market on the terms upon which both its supporters and opponents agree, namely, whether it does or does not provide economic well-being through the production and distribution of goods. Rather, we should evaluate it on precisely the terms that the counterculturalists struggled (mostly unsuccessfully) to enunciate, namely, whether it enhances life satisfaction and human development through the processes of working, creating, interacting, and desiring.

Lane has not been completely converted. No counterculturalist would agree that as between "relations with others" and work, the latter is "the major source of adult development" (p. 5). Nevertheless, his book rings with affirmations of the need for self-esteem, personal growth, and mutually satisfying relations. Similarly, no counterculturalist would turn, as Lane does, to the most rigorous experiments in cognitive psychology for evidence about human development. Still, he uses this evidence to judge markets according to the criterion that "the value of persons is as great as the value of commodities" (p. 7). Tom Hayden and Herbert Marcuse, move over!

In all seriousness, this is an extraordinary book in many ways. First, the sweep of its claims is breathtakingly broad and important: they will indeed revolutionize social science and social policy if given the attention they deserve. Second, an array of detailed analyses of social science literatures is brought to bear on the question of what actually happens as people experience the market. Lane analyzes hundreds of psychological experiments, opinion surveys, social theories, and philosophical treatises in the work of examining topics ranging from the meaning of rationality to the meaning of happiness, from the social role of money to the possibility of friendship. His ability to link these narrowly focused studies of particular locales to sweeping generalizations about human nature—and to do so mostly persuasively—is yet another extraordinary feature of *The Market Experience*.

It is surprisingly hard to summarize the flow of the narrative—as witnessed by the fact that the introduction lays out several lines of argument, none of which bear much resemblance to the actual order of the chapters. Lane explains why the book is not "linear" and is "reflexive," and his reasons both accurately describe and convincingly support his own project. Roughly speaking, *The Market Experience* begins with broad discussions of the meaning of "human capital" and of the importance of recognizing the interrelatedness of all forms of social engagement. It moves into analyses of useful and misleading ways of understanding the meaning of rationality and emotions and argues for the value of "cognitive complexity" as a substitute for rationality as a mark of human development. It then further elucidates the idea of human development by analyzing self-attribution (what political scientists more commonly call *efficacy*) and self-esteem. It moves at that point from individual traits to relational issues of friendship, market exchange, and the social meaning of work. From there, *The Market Experience* criticizes conventional treatments of money (the presumed reward of undesired work) and reformulates the real rewards of work (intrinsic satisfaction at a creative job well done, which might incidentally bring in some money). By this point, Lane is ready to analyze the meaning of happiness and the role of markets in producing it. He concludes with a brief sketch of the "elements of culture and society that promote and retard . . . economic reforms." A long, winding, and rewarding journey, with perhaps not a clear enough end.

Throughout the book, Lane refuses either to condemn or praise markets in simple terms. They are good at producing self-attributions (which are emotionally desirable even if empirically mistaken); they are misused to equate happiness with more money (above a certain level of well-being); their relation to self-esteem is complex. In general, markets do better at things they are not designed for (creating meaningful work and mutually satisfying interactions) than at things they are designed for (inducing people to work hard at hated jobs for monetary rewards). Over and over, Lane surprises with his iconoclasm and provokes with his cool, idiosyncratic, and well-defended assessments.

I cannot conclude without mentioning the most extraordinary element of *The Market Experience* (for political scientists, at least). Politics is almost totally absent from it. Lane hardly mentions the role of political institutions in shaping markets or in choosing among alternative systems of production and distribution. He barely refers to the role of markets in determining who gets political power or what people with political power can do in a given society. "Politics" appears once in the index, "power" twice, and "government" four times—this in a book crammed with a discussion of almost every conceivable form of human interaction. Ignoring politics was clearly a deliberate choice, and I wish Lane had told us why he made that choice. In my perhaps parochial view, the absence of any discussion of power or politics leaves a large hole in this otherwise comprehensive analysis of what markets can do or actually do and in any claim about how we should think about them. We are left with a final irony: the philosopher and economist Albert Hirschman tells us more about the interaction of politics and markets (in *The Passions and the Interests*) than does the political scientist Robert Lane.

Princeton University JENNIFER L. HOCHSCHILD

Redeeming America: Piety and Politics in the New Christian Right. By Michael Lienesch. Chapel Hill: University of North Carolina Press, 1993. 322p. $45.00 cloth, $17.95 paper.

This book examines the political thought espoused by leaders of the Christian Right in their published work. It is a welcome addition to existing scholarship, which focuses disproportionately on the activities, rather than the thought, of the Christian Right. It is also a corrective to a voluminous literature produced in different academic disciplines, which skewers the Christian Right without much pretense of taking its viewpoints seriously. Lienesch notes that the movement has often been "[viewed] as intellectually insignificant and dismissed as a moralistic abnormality" (p. 20). This book objectively probes the philosophic underpinnings of the Christian Right, expounding its worldview while also unmasking its inconsistencies and outlandish claims.

The introduction is a synopsis of the contemporary Christian Right. The reader familiar with the literature will not find much new here except for the argument that the Christian Right is more than a garden-variety social movement, because it offers a comprehensive worldview "based on beliefs and inspired by values" (p. 20). Another theme here is the periodicity of conservative religious political engagement. This discussion is informative, but others have examined the subject in greater detail—such as Ted Jelen's *Political Mobilization of Religious Belief* (1991) and Clyde Wilcox's *God's Warriors* (1992).

The next five chapters compose the heart of the book, looking at the Christian Right's political thought "in the context of a series of spheres, beginning with the self and extending outward in concentric circles to include the family, the economy, the polity, and the world (p. 21). The chapter on "self" focuses on the importance of the individual's converting to Christianity, with its concomitant political consequences. The notion of conversion explains a major division among religious conservatives between those restricting their focus to saving the individual soul and those extending the idea to redeeming a morally corrupt society. Conversion also accounts for the Christian Right's fervor: the imperfect person must constantly strive to overcome—his own sinful nature or society. This chapter draws heavily from the biographies of televangelists and probably takes their claims about their individual conversions too much at face value (but with no appreciable harm to the argument).

Chapter 2 focuses on "family," and it is a strong point of the volume. Lienesch relates the deep philosophic contradictions about gender roles. Husbands are called upon to be patriarchs but also sensitive partners; wives are to sacrifice for the sake of others but, in doing so, to exert control over familial relations (a classic case of assuming power by refusing power, à la *Marbury* v. *Madison*). Ambivalence also exists over sexuality (particularly as to whether women are temptresses or virtuous mothers), with marriage deemed a reasonable litmus test. Feminists seeking a careful rendition of the Christian Right's philosophic and pragmatic objections to their cause will find it here. In a separate vein, the author shows how parents hold conflictual attitudes toward institutions that shape the family. They attack public schools for their secular orientation, for instance, while making sure that their own children receive a sectarian, private education. The difficulty of reconciling literal Christianity with contemporary outlooks is skillfully covered in this chapter.

Chapter 3 uses the language of Reaganomics to describe views toward the economy. Christian Right leaders favor trickle-down economics because it recognizes the principle of creating more wealth to benefit all, rather than redistributing money to reward the indolent. They advocate a supply-side theology that justifies the accumulation of wealth as a reward for hard work and as recognition of God's blessing. Lienesch shows how parables are tortuously interpreted to rationalize wealth, while passages attacking the rich are glossed over. This chapter complements work on the awkward relationship between secular and religious conservatives, found in places such as E. J. Dionne, Jr.'s *Why Americans Hate Politics* (1991).

Chapter 4, on the "polity," analyzes the overtly political tracts written by religious conservatives. Many of those works interpret history, arguing that America is part of a providential plan gone awry as religious conservatives have left the political arena to secularists and humanists. (Hence the need to reestablish a "moral majority.") The most peculiar aspect of the Christian Right's political thought surfaces in this context, namely, the identification of a humanist conspiracy. Lienesch demonstrates that leading figures proceed beyond the proposition that secular ideologies have grown more important into the bizarre claim that a concrete number of humanists control public affairs. He further shows how that outlook abets different orientations: "At times, [Christian Right leaders] act as a morally conservative majority, committed to reclaiming mainstream values; at others, they are a militant minority, a pragmatic group of power politicians determined to implement their own political platform; at still others, they are a radical remnant seeking to overthrow the system" (p. 171). Torn between status as a latent majority or a victimized minority, Christian Right leaders present both scenarios. More attention to the type of audience receiving each kind of message would have further strengthened this presentation.

One other dimension of this chapter will intrigue political theorists and perhaps those attuned to politics in the academy. Christian Right leaders deplore the Enlightenment for advancing the causes of secularism and humanism, with Rousseau singled out for inculcating the separation of reason and revelation, as well as laying the groundwork for the French and Russian revolutions (pp. 159, 180). This body of work supports Lienesch's earlier claim that the Christian Right is more than a modern political force: it offers a worldview, complete with people for whom Locke and Rousseau are more important than Clinton. This theme also touches upon contemporary campus politics: Is the Enlightenment the flowering of knowledge, the birth of secularism as religion, or the inflation of the views of Eurocentric white men? In an odd twist, Christian Right leaders and multiculturalists seem to converge in their mutual antipathy for the traditional interpretation of the Enlightenment.

Chapter 5 focuses on the "world," and provides an exegesis of the idea of America as a "city on a hill." It does not break new ground, however—which is also the case with the chapter's anticommunist and eschatological themes. The brief conclusion, on the other hand, provides a thoughtful summary of the movement's past and future direction. Substantive notes and a solid index round out the book.

This book should be read by scholars of the Christian Right, both for understanding and for testable hypotheses. It may also interest those teaching American political thought. Lienesch has been immersed in the published work of Christian Right leaders, and he has produced an interesting and important book.

University of Maine MATTHEW C. MOEN

Monitoring Government: Inspectors General and the Search for Accountability. By Paul C. Light. Washington: Brookings, 1993. 274p. $31.95 cloth, $12.95 paper.

This book, by Paul Light, is a monograph on an institutional innovation in the federal government, namely, the centralization of auditing and investigation functions at the top level of executive branch departments and agencies, with such functions subordinated to statutorily empowered inspectors general. This institutional innovation was achieved legislatively during the late 1970s and implemented administratively during the Reagan presidency. Drawing on survey and interview data collected by Light, as well as the documentary record, *Monitoring Government* explicates the legislative and administrative processes through which offices of inspector general (OIGs) throughout the federal government became well endowed and institutionally secure. The book further makes a number of evaluative claims about OIGs and provides some supporting argumentation. Beyond this, the author endorses specific modifications in the institutional design of major OIGs in order to capitalize on their legitimacy, staffing levels, and civil service ethic. The book thus provides information that will be invaluable to scholars who seek to generalize about the politics of public management, and it will serve as a point of reference in debates about both the public policy emphasis on compliance-oriented accountability and the effectiveness of OIGs as a policy instrument.

The book's principal empirical generalization is that inspectors general have dedicated themselves to monitoring whether government employees, contractors, and others are complying with federal rules and regulations. In strategies reminiscent of the Federal Bureau of Investigation, many inspectors general chose to focus specifically on catching as many wrongdoers and as much wrongdoing as possible. Light explains the dual focus on compliance accountability and production of statistical accomplishments by analyzing the incentives at work in the federal political–administrative system. If OIGs' main objective was institutional autonomy vis-à-vis their respective departments, they had an incentive to please Congress and the Office of Management and Budget (OMB). Congress could grant them more autonomy by amending the Inspectors General Act; the OMB could override department-level staffing and budget decisions. Offices of inspector general were therefore rationally responsive to congressional and OMB interests. For Congress, the focus on compliance monitoring and "stats" production yielded recommendations for actions that (in comparison with rival approaches to improving accountability) were politically more palatable, jurisdictionally cleaner, less expensive, and quicker to implement. For the OMB under Reagan, outputs of compliance monitoring processes were considered evidence supporting the claim that the administration was winning the war on waste, fraud, and abuse.

A second major empirical claim is that statutory OIGs in departments and major agencies under Reagan became well endowed and relatively independent of the organizations they monitored. They had grown by almost fifteen hundred FTEs by 1986. Autonomy was enhanced by the OMB's sponsorship of inspectors general as a corporative group, allowing incumbent inspectors general across government to screen candidates for specific posts and permitting a substantial measure of self-regulation. Inspectors general retained the statutory right and obligation to report to Congress without the content cleared by department or OMB officials. In addition, OIGs enjoyed formal and informal means to override decisions made by—and circumvent obstacles imposed by—their department's own administrative staff functions (the only major exception being mandatory reliance on centralized functions for legal advice). Mainly on this basis, the author infers that "the government seemed better equipped in 1989 to catch wrongdoing, albeit after the fact, than in 1978" (p. 221). Under Bush, the autonomy of inspectors general vis-à-vis departments was diminished by changes in both the selection process and the OMB's structure and focus. However, major establishments' OIGs remained sizable and active.

Light proposes three specific institutional changes intended to enhance the autonomy of inspectors general vis-à-vis departments (an appointments process modeled on the Reagan administration's, a fixed term of office, and no pay-for-performance) and one change intended to enhance OIGs' capacity to conduct investigations (a statutory provision specifically granting their investigators limited power to subpoena testimony). The author argues that OIGs ought to become a principal home for evaluators within the bureaucracy. Among the grounds for this recommendation is the prescriptive claim that they should broaden their approach to achieving accountability and the empirical claim that they have the institutional muscle to secure the needed positions and depoliticize analysis. Although the author expresses serious doubts about the desirability and feasibility of this strategy for increasing and improving evaluative work within the bureaucracy, he concludes that given the current politics of public management, the strategy is nonetheless optimal.

Perhaps ironically, readers of *Monitoring Government* will remain uncertain, throughout the book, what Light wants to be held accountable for. It is plain that he intended to pledge himself to demonstrate the warranted assertibility of the two descriptive generalizations about inspectors general and their offices just discussed. However, he also states that their "eventual reliance almost exclusively on compliance in their search for accountability reveals a great deal about the politics of *administrative reform*" (p. 12, my emphasis). Even though other administrative reforms are briefly *discussed*, the reader is uncertain if it is fair to evaluate the book on how well the author formulates and defends descriptive generalizations at the level of administrative reform. The same kind of uncertainty bedevils the critical consideration of evaluative generalizations about OIGs. Light states that inspectors general and their offices "provide an opportunity to examine the consequences of eight specific 'innovations' in the search for accountability" (p. 2). (As revealed in subsequent lines, the statement not merely prefatory.) It is impossible to know what specific evaluative generalizations about OIGs Light proffers as warrantedly assertible (as opposed to simply plausible)—not to mention what standards of evaluative inquiry he wants the reader to accept. Uncertainty on this score is not resolved in the main body or in the notes. If the book is accountable solely for substantiating the two descriptive generalizations discussed, the research project is an evident success. The collection of data, the culling of information, and the framing of evidence is impeccable.

It may be that the author intended to provide a definitive evaluation of the implementation of the OIG concept. However, specialists in public management and policy analysis are bound to conclude that the work merely engages evaluative questions, failing to wrestle any of them to the ground. In conceptual terms, Light appears to conflate the effectiveness of the *policy* of reducing waste, fraud, and abuse in government with the *organizational* effectiveness of OIGs. Within a given policy frame (even a misguided one), OIGs may be evaluated favorably. Light clearly wants to make the case that the policy is largely ineffective (or even perverse), because overseers have reallocated resources from such staff functions as planning and evaluation units to OIGs. This case is plausibly made. However, once a distinction between policy and organizational effectiveness is drawn, it appears that the author barely engages the second issue. Four points are relevant. First, no information or commentary is provided about the basis upon which OIGs claim to have put funds to better use as a result of their operations. Since this category greatly surpasses criminal recoveries as a source of savings, one cannot begin to know whether OIGs' major indicator of effectiveness is more than mere rhetoric. Second, no consideration is given of whether inspectors general attempted to limit the adverse consequences on agency operations of their own investigations and auditing work. Third, operating and performance standards of the "compliance industry" were not brought to bear in making inferences about organizational effectiveness. Did OIGs implement formal or informal programs to encourage auditors working on civil enforcement cases to nominate cases for criminal investigation before they became stale? How much enforcement presence (an element of deterrence) was created as the result of auditing and investigation work? How efficiently was enforcement presence created—as compared, for example, with the auditing and criminal investigation functions of revenue agencies? How well did OIGs utilize the "visible odium" of criminal convictions for deterrence purposes? Finally, taking into account valid excuses provided by institutional and personal incentives, did OIGs focus effort in ways that could reasonably be seen as maximizing economy and efficiency (however defined)? Absent plausible answers to these kinds of questions, one remains ignorant about the organizational effectiveness of OIGs.

Harvard University MICHAEL BARZELAY

The Constitution and American Political Development: An Institutional Perspective. Edited by Peter F. Nardulli. Champaign: University of Illinois Press, 1992. 238p. $39.95 cloth, $14.95 paper.

The United States as a Developing Country: Studies in U.S. History in the Progressive Era and the 1920s. By Martin J. Sklar. New York: Cambridge University Press, 1992. 328p. $49.95 cloth, $14.95 paper.

These two books are fascinating contributions to the growing literature synthesizing politics and history, though each for very different reasons. Martin Sklar's *United States as a Developing Country,* is a collection of articles representing over 30 years of his work developing new perspectives about American political institutions. The panoramic view presented by one of our most provocative intellects includes his hitherto unpublished essay, "Dollar Diplomacy According to Dollar Diplomats," which dates to his Wisconsin University master's thesis in the late 1950s; articles published in the 1960s, and more recent contributions. In addition, he composed an original opening chapter especially for this volume, "Periodization and Historiography," in which he forcefully argues that U.S. society cannot be studied "without reference to its *political* history" but, rather, must be envisioned as a developing nation whose evolutionary stages reflect the common need of all industrializing societies to come to terms with the dual forces of "capitalism and socialism" (pp. 10, 36).

As we know from Sklar's influential *Corporate Reconstruction of American Capitalism, 1890–1916* (1988), he explicates the underpinnings of the American state in terms of market forces defining the parameters of political development. His collection of essays is a "must read" because it provides a more complete understanding of the nuances and scope of his own development as a noted scholar of American political change. Yet despite the author's commitment to political institutions, it leaves us somewhat in the dark about how major political structures are actually connected to political evolution over time. The book's index, for example, contains no entries for "Congress," "Supreme Court," or "Presidency." Similarly, while it lists only 6 pages in reference to some form of "constitutionalism," topics related to "corporatism" get 95, and "investment," 28. While this is not surprising, given Sklar's well-known proclivities, it nonetheless presents a somewhat skewed account of the political history of American institutional development.

To appreciate the conjunction between political structures and subsequent historical processes defining American state development, we would do well to turn to the excellent volume edited by Peter F. Nardulli, *The Constitution and American Political Development.* In an innovative enterprise, Nardulli gathered together "recognized American politics scholars" who were "not constitutional experts" (p. viii). He then asked this eminent group of nonconstitutional experts how "constitutional factors" influenced the development of Congress, the presidency, the bureaucracy, the Supreme Court, political parties, interest groups, and federalism in the context of American political history (p. viii).

The result is extraordinary. The essays are uniformly well written and informative, and they address the main theme with exceptional coherence. Taking, as a starting point, that the Constitution embodies "the external standards that define American ideals" (p. 15), each author analyzes the impact of constitutional provisions upon institutional components of the American political system. Superb chapters on Congress by David Brady and Charles Stewart trace with clarity and precision how partisan politics mesh with legislative structures to translate electoral preferences into legislated policies. Bert Rockman draws upon marketplace analogies to analyze insightfully the evolution of the presidency within the confines created by constitutional provisions, and Lawrence Baum and Kermit Hall provide incisive appraisals of Supreme Court activism and federalism, respectively, as vital component of public law—to cite just a few of the many fine essays in this collection.

The historical turn taken by many scholars interested in the confluence of politics and history extends state-centered analyses of political systems and their policy outputs. Launched by new institutionalism approaches a decade or more ago, the focus on how the presence or absence of state structures influences the policy articulation and accomplishments of societal actors not only "brought the state back in" but often resulted in significant cross-national comparisons between the United States and (usually) Western European nations. While often only implicit, these analyses had, as their subtext, historical premises about American political development that the current concentration on politics and history helps to explicate.

In this context, both of these books make important contributions. Martin Sklar highlights the relationship between the economic foundations of society and its political development, even as he all but excludes such basic legacies as the constitutional arrangement of political institutions. Yet it is this omission that informs Peter Nardulli's provocative collection. Those teaching graduate proseminars, therefore, would do well to consider both works. They feature contrasting themes, which, when taken together, broaden our understanding of American political development and the diverse intellectual landscape defining its terms of debate.

Northeastern University EILEEN L. MCDONAGH

The Dynamics of Ethnic Competition and Conflict. By Susan Olzak. Stanford: Stanford University Press, 1992. 271p. $32.50

Olzak offers an ambitious and insightful view into the workings of group conflict. Her contributions in this volume are both theoretical and empirical. She focuses her attention on the historical period from 1877 to 1914 in the United States. During this period, there were massive waves of immigration into this country, as well as internal migration. There were also periods of economic turbulence, accelerated industrialization, evolving and strained relations between races, and labor difficulties. There was violence as well, both white-on-black as well as against Asian and European immigrants. In short, this period contained a rich brew of conflict situations that makes it a perfect setting for the current study.

Olzak employs data from daily editions of the *New York Times*. She has collected a valuable body of data from a close reading of the actual newspaper articles rather than using the more limited information found in the *New York Times Index*. She identifies conflict situations from the largest 77 cities in the United States, and collects both qualitative (i.e., the type of conflict) and quantitative (i.e., how many people were involved) data for all observations. Olzak combines this information with additional social information relevant to the time period and pertinent to particular geographical regions. Her dominant empirical strategy is to use (wisely, in my view) event history analysis to unravel the major puzzles of the period.

The scope of Olzak's analyses is quite striking. She analyzes the influences of immigration and economic contraction on ethnically related events. She focuses particularly on labor unrest as it relates to violence against African-Americans. This later expands in the book to an analysis of lynchings and general urban violence. The competition for jobs is an area that adds to her economic analyses. Finally, in a chapter co-authored by Elizabeth West, Olzak analyzes the birth and mortality of ethnic newspapers in relation to incidents of violent ethnic conflicts. Her results throughout, and particularly in the chapter on ethnic newspapers, are not always intuitive. Indeed, it is precisely because she comes up with so many logical but nonobvious conclusions that her work is so valuable.

Olzak's basic argument is that competition is the driving mechanism behind most ethnic conflict. She challenges previous theories that suggest that ethnic conflict arises from things like relative deprivation, modernization and development, and assimilation difficulties that result in threats to an existing ethnically defined balance of power. One of her primary hypotheses is that ethnic conflict arises not from the separation and relative deprivation of groups, but rather from the breakdown of the former social order that kept the groups apart. It is when groups mix that they begin to compete. Essentially, they compete for the same limited resources, and Olzak's perspective is not unlike that of a typical population biologist who sees much of the world in terms of competing species.

Olzak's arguments lead to some highly interesting observations. For example, she claims that violence against African-Americans can result when another group (i.e., not African-Americans) migrates into an area, thereby increasing the competition for jobs among white Americans. Thus, it is not necessary for the in-migrating group to experience the conflict (although that may happen initially), even though that group's activities may be the ultimate cause of the struggle. Conflict with the initial immigrants can subside while whites escalate their attacks on African-Americans. All of this is connected to the idea of competition for resources (e.g., jobs) between members of differing ethnic groups.

From an empirical perspective, Olzak makes her points using both tables and some useful plots. I find her empirical analyses to be both competently executed and creatively designed. However, one of her primary contributions is in the realm of theory. Her view is one that is rooted in time-dependent dynamics, not static social conditions, and I am certain that all scholars will find something either to praise or to contest among her many findings and conclusions. Ultimately, she is not saying that humans fight because there are injustices in the world, but rather because the injustices are in evolutionary processes of decay that cause groups to interact in new ways. To use a meteorological comparison, wind (read "conflict") occurs not because there is a high pressure area next to a low pressure area. Rather, wind begins when the ridge that separates the two pressure systems begins to break down and the air masses from the two systems begin to mix.

Some may even find that Olzak's views challenge their basic notion of human rationality in situations of collective conflict, since her perspective could be interpreted as more oriented along the lines of stimulus-response than in action based on decision-making and the availability of information. In short, Olzak has written an extremely interesting book, filled both with the intelligent analysis of data as well as theoretical conclusions that are both provocative and compelling.

The publication of *The Dynamics of Ethnic Competition and Conflict* serves a useful secondary purpose. Many (but by no means all) of the results in this book have appeared in article form in a variety of settings. Olzak's prolific writings needed to be organized into a coherent thesis, and this book does this nicely. She has an interesting theory about group conflict, she argues her points effectively, and she leaves the reader with much to ponder. It is a book to be recommended to all those interested in the social structural mechanisms of volatile group interactions.

Emory University COURTNEY BROWN

Out of Order. By Thomas E. Patterson. New York: Knopf, 1993. 301p. $23.00.

This well-written book explores the mass media's influence in presidential elections. No previous study better documents the difference that media-dominated campaigns have made to American politics. Full of epigrammatic insights, this study combines exhaustive empirical research with trenchant normative observation. *Out of Order* argues that the media and especially television have replaced political parties as the chief screening agencies for presidential candidates. The results are baleful. The year 1960 was the turning point. Television news began drawing large audiences, and the parties started relying heavily upon primaries to select convention delegates. The parties abdicated much of their king-making function, the media generated more authority in the winnowing process. Simultaneously, they have increasingly portrayed presidential candidates as irredeemable liars and hypocrites.

Patterson argues convincingly that journalists fail to distinguish between trivial, politically necessary, or correctable dissembling (exaggerating opponents' liabilities, denying adultery) and lying that undermines the basis of elections. The latter occurs when candidates deliberately deceive voters as to their policy intentions. Patterson's research systematically reveals that contrary to the media's suspicions, elected candidates normally do keep their word: they pursue and often deliver the policies they promise.

According to Patterson, media coverage nonetheless assumes and conveys that essentially everything candidates say is motivated by politics. As such, journalists hold, it can safely be categorized as manipulative sloganeering. This assumption bolsters the the well-documented tendency for campaign journalism to focus on the horse race—who's ahead, who's behind, what strategies are working, what new strategies are planned. Even when reporters discuss issues, the view that candidates' positions are just part of the game plan permeates coverage. All this not only reduces the public's ability to learn about the candidates' public character, record, and policy plans, but it trains Americans in cynicism.

Among the book's most useful features is a series of tables tracing aspects of coverage from 1960 through 1992. One documents the growing analytical orientation of campaign news. No longer is a detached voice the norm. Reporters habitually air opinionated evaluations of candidates. The assessments focus not on substance but on the quality of campaign gamesmanship. The data suggest that descriptive material comprised over 90% of front-page coverage in the *New York Times* in 1960; by 1992, description accounted for under 20% and interpretation over 80%.

The historical-trend data also show that the proportion of stories framed in terms of policy has plunged from over 50% in 1960 to less than 20%, while game or horse-race coverage has risen from 45% to about 80%. During the same period, the balance of good versus bad news about candidates has shifted from 75% good in the earlier campaign to about 60% bad in 1988 and 1992.

There is always room for criticism of coding decisions: What exactly comprises good or bad news? Might the nature of good and bad news vary with candidates and campaigns? Is horse-race news not sometimes intertwined with issue coverage? But Patterson marshals much evidence supporting the basic validity of his argument. For example, in the same 1960–92 period, the proportion of the public responding unfavorably to both major party nominees has gone from about 20% to over 50%. The length of sound bites on television has shrunk markedly; and, Patterson shows, so has the length of candidate quotes in the *New York Times.* In all media, the derisive, prognosticating voice of the reporter is subsuming the policy voice of the candidates.

One specific example that Patterson cites is a report on candidate Bill Clinton's 1992 campaign speech to a labor group. A network report featured a sound bite of Clinton saying, of the North American Free Trade Agreement (NAFTA), "I'm reviewing it carefully, and when I have a definitive opinion, I will say so. It's a very long and complex document." The reporter then says: "Time out! Clinton has a reputation as a committed policy wonk who soaks up details like a sponge, but on an issue which will likely cost him votes no matter what side he takes, the onetime Rhodes scholar is a conveniently slow learner" (p. 6).

Patterson points out that the NAFTA document was 1,078 pages long and argues: "To suggest . . . that the 'policy wonk' Clinton should know every part of it and flatly accept or reject it in its entirety is disingenuous. Worse than that, it is fatuous. It assumes that having a fixed opinion on every aspect of every issue . . . displays true leadership, and that any hesitation or objection on a candidate's part is a practiced deception" (p. 16). This passage exemplifies the straightforward and lucid normative analysis that the book weaves through its empirical narrative.

The author believes that because the press filters innovative policy proposals through the cynical horse-race schema, the news often punishes candidates for talking in earnest about policy. The coverage tends to portray debate about substantive proposals as campaign-threatening controversies. A classic example was McGovern's proposed negative income tax: originated by Milton Friedman, the media treated this proposal as a gaffe, helping to stigmatize McGovern as a leftist radical. At the same time, notes Patterson, the media frequently chastize candidates for avoiding honest, original, detailed issue stands. Damned if they do and damned if they don't, candidates begin resembling the cautious beasts that the media construct.

No book is perfect. While Patterson's own content-analytical material is enlightening, he sometimes buttresses his claims with others' questionable data. For example, he cites research claiming that only 2% of the Fall campaign coverage in 1992 focused on the "character" issue for Bill Clinton (p. 195). Others might have coded the many mentions of "family values," "trust," Gennifer Flowers, "Slick Willy," and similar phrases as oblique or direct comments on Clinton's character. The book would have been even stronger had Patterson stuck to his own research data, the most pertinent and convincing. Nevertheless, his eloquent reasoning and masses of evidence establish a compelling case. Patterson is careful not to blame individual journalists or news organizations for the state of electoral politics. He recognizes the interlocking structural causes of the malaise. He notes that journalism is only part of the problem and that the parties' decline has thrust upon the media a function they simply cannot carry out. The book's proposed remedies are, as always, debatable, but they are worth considering. If I were to recommend one book among the dozens on media and U.S. presidential elections that have appeared in recent years, this would be it.

Northwestern University ROBERT M. ENTMAN

The Supreme Court and the Attitudinal Model. By Jeffrey A. Segal and Harold J. Spaeth. New York: Cambridge University Press, 1993. 390p. $54.95 cloth, $17.95 paper.

This is an important book by an important pair of scholars of the Supreme Court. It has already engendered a great deal of controversy, even among those who generally agree with the line of reasoning; and it will accordingly create even more controversy as the proponents of other approaches to the study of the Supreme Court find and read this book. Those who adopt this model as a guide to understanding the Court will swear by this book. For others who take sharp issue with it, I suspect it will become a book many will love to hate. Few who read it will come away with neutral feelings.

Segal and Spaeth take up the cudgels for what they call the *attitudinal model* of decision making on the Court and take direct aim at competing understandings of what motivates the justices to behave as they do. The main competitor is what they call the *legal model.* The attitudinal model, according to Segal and Spaeth, "holds that the Supreme Court decides disputes in light of the facts of the case vis-à-vis the ideological attitudes and values of the justices" (p. 65). By contrast, proponents of the legal model see judicial decisions as a response to the facts of a case in light of "plain meaning, intent of the framers, precedent, and balancing of societal interests" (p. 64).

So what motivates the members of the Court? Is it precedent, plain meaning, and the rest of the litany, or it is raw policy preferences? If I had to choose between the two, I would indeed take the latter. But Segal and Spaeth's views, at least initially, are extreme, with few subtleties. They really do not set up any realistic competitor to their model of decision making. I can think of no political scientists who would take plain meaning, intent of the framers, and precedent as good explanations of what the justices do in making decisions. Some political scientists and many lawyers think it would be nice if justices did follow precedent, the framers' intent, and so on; but that is another matter. If there is no position other than the attitudinal model and the silly formalism of the legal model, the debate is over. Yet no one who has taken Introduction to American Government, read *Marbury* v. *Madison,* or witnessed the fights over nominations to the Court during the Reagan and Bush years is going to ascribe to the legal model. Thus, it is really not a worthy foe, and much of Segal and Spaeth's discussion of it is beside the point. Segal and Spaeth's real foes are the many political scientists and lawyers who would belittle the analysis of votes and say that we have to look at opinions as a whole. But, peculiarly, the authors do not make them the main target of the book.

Segal and Spaeth's formulation of the attitudinal model strikes me as too blunt. Justices, like other political actors, are not free to translate their preferences directly into policy in any and all situations. Instead, the justices maximize their policy preference under the constraints of law, policy, and custom. Liberal justices, for example, do not grant certiorari on trivial cases simply to reverse a conservative outcome in the lower courts: they grant it on cases in which some live legal issue is present and in which some plausible case for meeting the Court's rules can be made. Of course, this is true of other political actors; and the distinctions many make among judges, legislators, and bureaucrats are exaggerated. All policymakers work within constraints; and, in this sense, judges are no different. Later in the book, Segal and Spaeth seem to take constraints into account; and, of course, they know that the justices do not simply enact their preferences. But the blunt formulation of the attitudinal model pops up again and again.

Chapters 4–7 constitute the heart of this book, the main evidence for the attitudinal model. In these chapters, Segal and Spaeth take up and present systematic evidence on staffing the Court, gatekeeping, decisions on the merits, and the distribution of opinion assignments. The evidence is plentiful and convincing.

The chapter on staffing shows how ideological considerations come into play from the very beginning, from the choice of the president all the way through the decisions of senators to vote for or against a nominee. The larger the ideological distance between a senator and a nominee, the likelier he or she will vote against a nominee. Questions about the qualifications of a nominee make a difference, but ideological distance magnifies the effect of this variable; that is, the larger the ideological distance between nominee and senator, the bigger the difference made by questions about qualifications.

In the chapter "Getting into Court," which is largely a review of prior work, Segal and Spaeth present impressive evidence of the ideological nature of the justice's decision making. For example, a justice is substantially more likely to vote to grant certiorari who disagrees with the outcome in the lower court. For decisions on the merits in the law of search and seizure (chapter 6), Segal and Spaeth show the strong impact of ideological considerations above and beyond the factual circumstances of cases. In chapter 7 ("Opinion Assignment and Opinion Coalitions"), they offer a wealth of evidence on the impact of ideology on how the chief justice distributes opinions among his colleagues. For example, although chief justices strive for equality of workload, they tend, on average, to assign more opinions to those closer to them ideologically and fewer to those furthest from them.

I do not see how anyone can come away from these middle chapters without granting the tremendous impact of the ideological positions of the justices. "Ideology," "attitudes," "policy preferences," "single-minded seeking of policy"—whatever one wishes to label this phenomenon—pervade the decisional processes of the Supreme Court. Thus, if we take the purposiveness of judicial behavior as the basic proposition of this book, I think the authors have succeeded.

Segal and Spaeth, in my view, try to do too many things here. To account for every facet of the Court in an empirical fashion in one book is a tall task: any one part could easily serve as the fodder for several monographs. Their insistence on full coverage—from staffing to compliance—sometimes precludes the full development of an aspect of the Court. The large number of topics makes for a distracting busyness. Then, too, some of the chapters do not fit well into a book of this kind. Segal and Spaeth set out a theoretical framework juxtaposed against the legal model and test it against several sets of data. Then, in the middle of the book, they give us a political history of the Court to show how ideological considerations have underpinned its decisions. I would be the first to sign up for the proposition that ideological considerations have, from the beginning, played an important role in the Court, but this short history proves nothing. Actually, this is the standard stuff of political-science reading of constitutional law going back at least to Corwin and Powell. If we were to look back at the first editions of Mason and Beaney's *American Constitutional Law* and Cushman's *Cases on Constitutional Law,* I would bet a large amount of money that we would find similar accounts of the same decisions. Similarly, in two later chapters, Segal and Spaeth go through the evidence on compliance with, and implementation of, the Court's decisions and the relationship between the Court and other actors in the political system. In chapter 9, on compliance and implementation, Segal and Spaeth review the existing evidence; in chapter 8 they present a number of descriptive tables. Neither of these chapters has much to do with proofs of the attitudinal model of judicial decision making. Perhaps the real problem is that this is both a scholarly monograph and a textbook. At least it has the components of both kinds of books.

Despite my reservations, this is a fine and important book, one I will certainly consult often. There is much to commend this book. First of all, it contains a wealth of information about the decisional processes of the Court, from the legal framework through the nitty gritty of voting, opinion assignment, and the like. Oddly, given the theoretical stance of the authors, this book is particularly strong on the legal framework of decision-making in the Court. Second, the authors adopt a clear theoretical stance. No theoretical waffling mars these pages. One may or may not agree with that stance, but it is out there, along with the data to support it; and others now have a clear target. Third, Segal and Spaeth use this book to bring together a great deal of work published in a variety of journals and to provide new data and results on a number of points. Fourth, since the Supreme Court Data Base Phase I supports most of the analyses, anyone who wishes to challenge the results or conclusions or to extend the work can easily do so with this public dataset.

Ohio State University GREGORY A. CALDEIRA

The Politics Presidents Make: Leadership from John Adams to George Bush. By Stephen Skowronek. Cambridge: Harvard University Press, 1993. 526p. $29.95.

What are the determinants of presidential leadership? Why are some presidents widely considered "great," while others range in historical reputation from mediocre to disastrous failures? These questions have, in one way or another, been central to the literature on the presidency, some of it centering on its constitutional function, some on the getting and keeping of real-life power, and some on the psychological qualities of the men who hold the office. Quite a bit of this literature has been framed in terms of a great wall of separation between the modern presidency since 1933–45 and the (mostly irrelevant) presidencies before then.

For Stephen Skowronek, the fundamental issues are embedded in a Constitution that, unlike society and economy in the United States, (and, for that matter, unlike the empirical operations of politics) is, in its foundations, unchanging, static, timeless and "nondeveloped"—the core of what Samuel Huntington has called the American "Tudor polity." In this world of separate powers and clashing ambitions linked to "the prerogatives of the place" (as Madison put it), the issues confronting presidents go back to the very beginning and not just to 1933 or so. More than that, there is a dialectic at work: the interplay between constitutional nondevelopment and political (and other) development in the United States produces regular cycles. Regime orders come into being in the wake of a massive, crisis-laden upheaval sequence. They persist at first robustly and then, over the longer term, more and more vulnerably as intramural infighting takes its toll and the development of society and economy creates new problems and cleavages that the existing order is ill equipped to meet. Finally, each in its turn, perishes in the next punctuational-change crisis, to be replaced, out of the ruins, by the next regime order and its transformational (Skowronek: "reconstructive") leadership. Such cycles are iterated across the entire span of chronological time. This means at least two things: (1) the contexts of presidential power are anchored in "political time," and (2) presidents occupying the office in comparable moments in political time may be profitably compared with one another, even though they may be separated by decades in the passage of chronological time.

This portion of Skowronek's model in principle permits classification of American presidents from the federalist era to the present day on two dimensions: their affiliation with, or opposition to, a given regime order and the extent to which that order is robust or vulnerable. This, in turn, gives us a fourfold classification of individuals in their contextual and political-time setting that has been well known in the field since the author zpublished his famous article on in 1986. The four types of political practice are (1) *reconstruction* (the great regime overthrowers and builders), opposed to an acutely vulnerable order; (2) *articulation,* affiliated with a robust order; (3) *disjunction,* affiliated to a collapsing order; and (4) *preemption,* opposed to a robust order. In the bulk of the text, the author systematically examines presidents and their contexts of action in the first three categories, from Thomas Jefferson's reconstruction to Jimmy Carter's disjunction.

But something else is superimposed. What forms the driving force behind presidential inputs to the world they inherit and attempt to shape is, in the first place, the inherently disruptive force of the office and its equally inherent hostility to all governing arrangements existing at the time that a given president first takes office. The keynote here was given by James K. Polk: "I intend *myself* to be President of the United States." If, as

Skowronek says, the office is simultaneously order-shattering, order-affirming, and order-creating in its properties, even weak incumbents tend to create significant disruption as they seek to affirm their own indentities in office. A classic case, discussed in detail in these pages, is the disjunctive presidency of Franklin Pierce. Order shattering is never far away.

Beyond that, there is not only political but secular time. If political time centers on presidential power, secular change in the mode of government operations affects, cumulatively, presidential authority. This unidirectional evolution falls into four eras that in general (if not always in detail), would probably gain wide consensus: the Patrician Era (1789–1832), the Partisan Era (1832–1900), the Pluralist Era (1900–1972), and the Plebiscitary Era (1972 to date). Thomas Jefferson is the purest and most complete example of reconstructive leadership because he operated in a near void of organized opposition at a very early moment in the development of social (and thus governmental) complexity. Over time, the growth of this complexity has produced a government that is so "thick" on the ground that its inherent resistiveness to presidential initiatives has made presidential achievement of reconstructive purposes more difficult and problematic with each new iteration of the power cycle. By the time we reach the present, *impasse* becomes the consequence—a major feature of the discussion of the presidencies of Ronald Reagan and George Bush, with which the work draws to a close.

Without any question, *The Politics Presidents Make* is the most ambitious effort at sustained pattern recognition ever attempted in this area of study. The overwhelming bulk of the book consists of a detailed, "thick-description" effort to concretize the categories by looking at presidents in action in both political-time and secular-time contexts. It is a truly extraordinary effort at integration, synthesis, and comprehensive pattern recognition amid the myriad details of the historically contingent and the diversity of character, talents, and virtues of the men who served in this role. In general, at least for me, the argument works. Skowronek is remarkable not only for his erudition and accuracy in detail but also for the subtlety of his reflections on his cases. This is a work that is striking in its sensitivity to nuance and—above all, perhaps—in its keen awareness of the ironies of presidential action. At very many points, quite new analytical possibilities are raised and evaluated. To take just two examples, presidents in a disjunction situation recurrently tend to reify technique as substance becomes impossible to handle, and presidents pursuing the politics of preemption recurrently tend to risk or achieve de facto or *de jure* impeachment.

Needless to say, this study, important as it is, does not lie beyond criticism. Any effort to construct classificatory categories of this type is likely to disclose concrete cases that just do not fit. Skowronek himself identifies "three hard cases": Cleveland, Coolidge, and Eisenhower. While I see little difficulty in classifying Coolidge's general place in Skowronek's greater scheme of things, Cleveland's case is somehow associated with a realignment in which (uniquely in American history) the forces demanding change were defeated. Eisenhower "ought" to fit as a preemptor (the New Deal order was still robust, and he and his party were opposed to it); but he did not behave that way in office and also lacked the modal preemptor's temperament. (In earlier times, one could say the same of Millard Fillmore.) Real life is likely to produce impure mixtures in concrete cases that classification schemes have difficulty accomodating. Yet if the model does, in general, seem to describe real-world trends and tropisms, such "hard cases" suggest less the need to discard the former than to work through the latter's implications.

Two more serious questions arise, both centering on preempting presidents, whom Skowronek calls "the wild cards of American politics," who have historically come to power because the normal screening or gatekeeping machinery for presidential selection has broken down for one reason or another. Inherent presidential disruptiveness in these cases has often verged toward the boundaries of the constitutionally permissible: Richard Nixon is just the latest of such cases in Skowronek's scheme. Yet the author, at the end, calls for a presidential strategy of "perpetual preemption" henceforth. In this context, it is particularly to be regretted that this book contains no detailed examination of at least one representative of this fourth category. To be sure, pretty much by definition, preemptors stand, in a sense, outside the cyclic pattern. But these cases illuminate issues directly specific to the author's closing argument, and at least one of them (Nixon?) should have been included accordingly.

Skowronek is not alone in concluding that within the past quarter-century, changes so fundamental in the organization and articulation of American politics have occurred that traditional historically derived precedents and models are becoming irrelevant to understanding what is going on in the here-and-now. From his perspective, the thickening of government has reached the point that presidential reconstructive activity is now, or soon will be, out of the question. Incumbents keep acting on cue from an old script but with diminishing effectiveness and increasingly bizarre consequences. Better, then, that they should give up acting out such roles and the public give up waiting for "Godot"—a new reconstructive president to bail them out of the mess their public affairs have got into.

Each president should thus now become his own preemptor. (Is Bill Clinton actually following this new script?) From such a change, a whole catalog of desirable consequences will follow: "Once shorn of the roles acribed to them in political time, leaders will in fact have little else to rely upon but reason, talent, ideas and character" (p. 445). In short, Skowronek's subtle dialectics of presidential power sum to an even more striking dialectic: the negation of an analysis that has occupied more than 400 pages up to this point.

One finds oneself struggling with two reactions to all this. First, one wonders, preemption *for what*—in behalf of what substantive values and concrete interests and coalitions in the society that elects these people every four years? In general, there is a tendency—ever more marked as we approach the present in these pages—to present an analysis that is so Oval Office–oriented that still-vital contexts of presidential action such as party and partisanship virtually disappear from view. And we do have that old problem with preemptors that Skowronek clearly identifies: How does the system keep the power drives of these figures within constitutional limits, particularly if the constraining power of other institutions continues its decay?

Second, how sturdy a support can this ideal preemptor gain from his pure qualities of "reason, talent, ideas and character"? They were just John Quincy Adams's frame of reference for conducting his presidency. They seem more generally (as proposed unmediated drivers of the enterprise) to reflect our frequent preoccupation with eliminating politics to achieve pure policy "in the public interest"—a Progressive Era impulse that is with us still. While many factors were involved in Adams's defeat for reelection in 1828, his efforts to remain far above "mere politics" in the conduct of his office helped mightily to do him in. If there is any one sentiment among the enlightened that ought to be given up as we move into the next century, it is not the fixation on "great presidents" as saviors but the striving finally to achieve some nonpartisan, nonpolitical ideal for government and its officials. Short of dictatorship, politics cannot be gotten rid of. For this reason, the preemptive virtues that Skowronek identifies in his heroic effort to find a way out of the present impasse would seem to be as effective a ticket to quick retirement for incumbents embracing them as it was for Adams.

All that said, however, it should also be said that any weaknesses this book contains pale into insignificance when measured by the magnitude of the accomplishment it represents. This is a magisterial work, one of the most important studies of the presidency—indeed, of American politics—ever written. Stephen Skowronek has made the most persuasive of cases for the value and significance of work in the American political development genre. He makes this case by doing the work and by revealing, in the process, a dense network of connectedness in action and context that would otherwise be inaccessible. His analysis of the impact of secular time on the presidential power cycles of political time provides (in addition to other things) a new and deeper insight into the present era's political impasse. At the end of the day, he comes very close to identifying the root problem affecting presidents, which prompts him to the sort of escape he then suggests. This is the all-important fact that the Constitution is unchanging and nondeveloped, while at all times intersecting with a social, economic, and political world that has undergone incessant development from the beginning. The whole work may be read as an extended, powerful, and penetrating meditation on some of the global consequences of this fact.

University of Texas, Austin WALTER DEAN BURNHAM

The Scar of Race. By Paul M. Sniderman and Thomas Piazza. Cambridge: Harvard University Press, 1993. 212p. $18.95.

It is not possible to understand American politics without eventually engaging the question of race. Of course, the centrality and character of debates on the status of African-Americans in the polity have varied enormously. Yet since before the founding of the republic, the "race problem" has been near the core of American national politics. Issues of race figured in the drafting of the Constitution, the enslavement of blacks weighed upon the consciences of Washington and Jefferson, Tocqueville's powerful chronicle of American society dealt extensively with race, a struggle between a race-based slave economy versus a free-labor economy precipitated the deadliest war in the American experience, and a range of constitutional debates (e.g., the scope and intent of the Fourteenth Amendment) and modern-day civil rights protections (the Civil Rights Act of 1964 and Voting Rights Act of 1965) can be traced to efforts to protect the status of African-Americans.

In the long shadow of the American political history of race, Sniderman and Piazza's *Scar of Race* aims to tackle afresh the linkage of prejudice and public opinion. Their focus is on the modern era. Their method is the sample survey conducted by phone, both local (San Francisco Bay Area) and national in scope. The research is driven by an effort to understand whether the politics of race has somehow changed. To assure wide accessibility, the data reporting is deliberately kept simple, rarely reaching beyond figures or tables based on simple percentage comparisons or correlations. On the whole, Sniderman and Piazza's effort is, variously, illuminating and clever; yet, on occasion, it disappoints and frustrates. Let us consider several useful contributions made in *The Scar of Race.*

First, the research helps to specify the current complexity of the issues involved in racial politics. At an empirical level, Sniderman and Piazza find no tight clustering of key issue attitudes. An individual's opinions on affirmative action in employment are not likely to be interchangeable with the same person's views on open housing laws or level of economic assistance, if any, that government ought to provide to black communities. The relative lack of constraint immediately implies that there is no single, profound thread of prejudice that binds together the views of white Americans on these public policy questions.

Second, the book is a useful normal-science extension of the use of experiments within surveys. Following the lead of Howard Schuman, who first applied survey-based experiments to theoretical questions in the study of contemporary racial attitudes, and the complex survey vignette studies of Peter Rossi and his colleagues, Sniderman and Piazza exploit the power of modern computer-assisted telephone interviewing systems to develop ever more complex experimental designs. One clear lesson of their efforts is that survey questions directed at delivering benefits to a black individual do not elicit a racial double standard in behavior of the type that emerges strongly when benefits are proposed to be delivered to blacks as a group.

Third, the book makes clever and often enlightening use of a measure of anti-Semitism as its bedrock indicator of prejudice. Doing so simultaneously reveals the modern political potency of prejudice and escapes entirely the analytical ambiguity of meaning that often attaches to questions with explicit race-based content. Fourth, the book is one of several recent efforts to reintroduce a concern with stereotyping into sample surveys. It provides valuable data on the persuasiveness and the delimited (but quite real) political effects of antiblack stereotyping. Fifth, Sniderman and Piazza undertake a very provocative examination of the malleability of opinions. They use counterarguments to attempt to gauge the fixity of opinion on race. The results show a potential for great movement, although this movement has a clear-cut predominant trend.

Most frustrating, however, among the weaknesses of *The Scar of Race,* is a serious misreading or misinterpretation of the *new racism* thesis as advanced by such scholars as David Sears, Donald Kinder, and John McConahay. Sniderman and Piazza typically treat the new racism thesis as contending that core American values, such as individualism and the work ethic, rather than constituting weapons against racism, are now the main engines of racism and that this new racial bias operates with special force among political conservatives. Neither component of the argument seems a fair depiction of the theory advanced by proponents of the new racism thesis. First, the latter maintains that a new form of expressing racial animus has emerged. A hallmark feature of this new racism is a more explicit concern with such values as the work ethic, though this is intimately fused with feelings toward blacks. Sniderman and Piazza's own research strongly supports at least part of this claim. For example, they show that even among the highly educated, believing that "if blacks would only try harder, they would be just as well off as whites" or that "most blacks on welfare programs could get a job if they really tried" are powerful direct determinants of attitudes on economic assistance to blacks—exactly as the new racism thesis would predict.

Furthermore, to address the second component of Sniderman and Piazza's treatment of the new racism thesis, I know of no survey-based research suggesting that this new racism operates with special or differential force among ideological conservatives. Hence, while it is quite useful to show that whatever antiblack bias exists when policies focus on group benefits is not unique to conservatives, this finding does not challenge the core of the new racism argument. The new racism thesis has many weaknesses, but this research has not taken us far toward resolving them.

Less troubling but still problematic is the inattention to group conflict theories as an alternative to both the new racism thesis and Sniderman and Piazza's politicization-of-race theses about modern race politics. Conflict models have considerable power to explain the difference found between popular receptivity to individual benefits versus collective or group benefits. Moreover, it might help explain why the tests of opinion malleability find the greatest movement on the group benefits question and that the trend of movement is disproportionately from pro- to antibenefit delivery.

Despite several limitations, *The Scar of Race* adds a needed perspective. It will advance discussion among specialists in the area and contribute to current efforts to understand the tenacious forces of race and racism in national politics.

University of California, Los Angeles LAWRENCE BOBO

COMPARATIVE POLITICS

Hostage to Revolution: Gorbachev and Soviet Security Policy, 1985–1991. By Coit D. Blacker. New York: Council on Foreign Relations Press, 1993. 268p. $16.95 paper.

Coit Blacker has produced a compact and very readable book that reviews the turbulent changes in Soviet foreign and security policy under Gorbachev. He writes in a well-organized and accessible manner, effectively using subtitles and verbal "road maps" to help the reader along. On the whole, Blacker presents a balanced description of the Soviet policy revolution that led to the end of the Cold War. After reviewing the Brezhnev legacy, he examines the effect of Gorbachev's "new thinking" in four areas: the conceptual basis of Soviet policy, military doctrine and strategy, arms control, and civil–military relations. In both style and content, the book is quite suitable for an upper-level undergraduate course on the history of Soviet or Cold War foreign policy.

At the same time, *Hostage to Revolution* is much weaker at an explanatory level, doing little to advance the scholarly debate. Despite the author's claims in the book's introduction, he presents a rather standard Sovietological analysis. Blacker has read a wide array of (translated) Soviet sources and, based on these and his understanding of the changing content of policy, offers a plausible reconstruction of Gorbachev's motives and reform strategies. Keeping with the Sovietological tradition, the book is also for the most part atheoretical. In other words, Blacker is at his best describing the content and implications of the Gorbachev-era changes in security policy. He is much weaker at explaining the why and how behind the reforms.

Indeed, Blacker's explanatory focus—economic crisis as the motor of change—is so simple and self-evident that the reader is left with a lingering question of "So what?" What insights are gained from an argument of this sort? Blacker rightly criticizes (in both his introductory and concluding chapters) international relations theory as being poorly equipped to anticipate or explain peaceful systemic change such as that brought about by the Gorbachev reforms. Yet he does not offer or elaborate a domestic-level alternative of his own, except to suggest that "economic crisis" is central and all other explanatory factors are essentially derivative (e.g., pp. 190–91).

As Blacker's story unfolds, we are told at various points that ideas matter, institutions are important, and leaders learn. Who would disagree? The analytic challenge is to explore the conditions under which such factors become relevant. To do this, however, requires greater attention both to domestic politics and theory. How and why did certain ideas reach Gorbachev and come to inform the content of his "new thinking"? If the old ideas on international politics were so deeply embedded in the pre-Gorbachev system (as Blacker persuasively argues), how could they be overturned with such relative ease once a new leader came to power? Blacker is right on the mark to argue that economic crisis "opened up" Soviet politics. To stop there, however, is to leave many of the most interesting questions unanswered.

It is fair to ask whether the domestic political process under Gorbachev could have been explored in the detail necessary to address such questions. The answer is *yes*, but not with the sources Blacker has utilized. For reasons that are not entirely clear, he has chosen to rely almost exclusively on U.S. government translations of the Soviet media. Aside from the evident problem of selection bias (a point Blacker never addresses), reliance on such materials intended for wide consumption seriously hinders the author's ability to reconstruct the process behind the "new thinking."

Much, in fact, has been revealed through Soviet/Russian materials not typically translated by the U.S. government. One scholarly journal, for example, has published the transcripts of the Reykjavik summit, a consideration of which would have strengthened Blacker's interpretation of this key episode (pp. 104–8). More important, a number of key figures have published memoirs (former Chief of the General Staff Akhromeyev, academic specialist Georgiy Arbatov, advisor Anatoliy Chernyayev, and former Politburo members Ligachev and Ryzhkov, to name only a few). While such works must be read with caution because individuals maneuver to secure their place in history, they nonetheless offer many remarkable insights. Beyond written works, one can, of course, "go to the source" and interview the architects of this remarkable revolution. While Blacker mentions his many helpful conversations with Soviet specialists (p. 8), his notes give no sense of what he gleaned from the interviews or how such information was integrated into the analysis.

To appreciate how neglect of domestic political process hinders Blacker's analysis, compare Gorbachev's foreign policy with that of post-Soviet Russia. Economic crisis, Blacker's key explanatory variable, is present in both periods. Yet in today's Russia, the "new thinking" has come under withering attack, while a different set of ideas more in keeping with traditional notions of power politics has come to the fore. Russian foreign policy, reflecting this combination of old and new, has become a strange mixture of realpolitik and liberal internationalism.

How to explain these changes? Clearly, part of the answer is the dramatically different international political environment of post-Soviet Russia (especially important here is what Russians call the "near abroad"—the successor states to the USSR). Equally important, however, are fundamental changes in the domestic setting; the process has opened up in ways unheard of even at the end of the Gorbachev era. Indeed, in Russia today, the problem faced by promoters of new ideas is not getting access to top decision makers (as it was in the USSR) but ensuring, once they reach elites such as Yeltsin, that the ideas have some lasting influence on policy.

Blacker's main concern, however, is the Gorbachev period, where the challenge is to get some sort of analytic leverage on an extraordinarily complex set of changes. The way forward is to incorporate, and expand upon, the work of scholars such as Matthew Evangelista and Jack Snyder, both of whom have sought to build much-needed bridges between international relations theory, comparative politics, and "Soviet" studies. Indeed, if recast with more explicit attention to theory, Blacker's study would have been much sharper analytically, with the potential for contributing to several different research programs in international relations and comparative politics. These include work on policy ideas, civil–military relations, negotiation/bargaining theory, and (more generally) domestic sources of state behavior. As written, however, the book leaves the reader to tease out any connections to the broader literatures.

In short, Blacker has written an informative if limited book. Students and members of the policy community will find much of use here. *Hostage to Revolution* is a good account of six years of revolutionary change. However, the reader who is interested in new interpretations that draw upon new source material and contribute to the broader social science literature will have to look elsewhere. Perhaps reflecting a field ("post-Soviet studies") in transition, Blacker's study is, ultimately, a mixture of good and bad. It is better than many Sovietological analyses yet still falls short on both methodological and theoretical grounds.

University of Pittsburgh JEFF CHECKEL

Parliamentary Change in the Nordic Countries. Edited by Erik Damgaard. Oslo: Scandinavian University Press, 1992. 224p. $39.95.

Coalition Politics and Parliamentary Power. By Mats Sjölin. Lund: Lund University Press, 1993. 230p. $21.50 paper.

These recent books on parliaments and parliamentary practices are the products of a persistent endeavor, on the part of some Nordic scholars, as well as research milieus, to study the impact of institutional forces in political life. Although clearly not flying the flag of the behavioral movement during the sixties and seventies, they continued presenting the kind of basic research that other scholars took for granted and that today enjoys a modest revival among Nordic political scientists. Damgaard's edited volume and Sjölin's monograph are different both in scope and intent and will have different value to different readers. They share, however, the same analytical background in contemporary comparative parliamentary research—even though the moorings are not quite similar.

In the Damgaard volume we are presented with accounts of recent parliamentary trends in Denmark, Norway, Sweden, and Finland sandwiched between an introduction and a comparative analysis, both produced by the editor, who is also responsible for the chapter on Denmark. The analysis of the three remaining parliaments are written by country specialists: Dag Anckar, Hilmar Rommetvedt, Anders Sannerstedt, and Mats Sjölin. According to the editor, the purpose is to analyze important trends in the parliamentary development of the Nordic countries during the past few decades. Criteria for "importance" is partly centrality within the comparative literature discussions, partly political importance within the countries in question. In practice, the "decline-of-parliaments" thesis functions as a kind of overarching point of reference, even if explicitly "not the main object" of the analysis (p. 16). The editor is also evidently skeptical as to its research potential.

Parliamentary analysis deals with both internal developments in parliament and its relations to the external world, particularly the executive. Starting with Danish experiences during the 1980s, contextual particularities immediately create problems for comparative analysis; for these experiences are indeed quite remarkable to those used to a fairly straightforward distinction between government and opposition. In Denmark during the Schlüter "four-leaf clover" government (1982–88), the very terms evaporate when analyzing the dealings in the Folketing. There were "alternative majorities" in several policy areas—most notably in security matters. this means that the minority coalition government, headed by the conservative Schlüter, accepted its minority position in several fields and let the "opposition" determine "government" policies. The era of "the governing parliament" came about because one of the parties not in government—the radical liberals—wanted the government to continue and the government was willing to pay the necessary price.

The chapters on the four country developments outlines the major parliamentary trends over the last two decades. They all focus on changes along the parliament—government axis, as seen from parliament. The historic accounts reveal that the old label for the Scandinavian region ("consensual democracies") has to be reconsidered. In short, there is more open and fierce politics on the parliamentary scene than there used to be in the 1950s and 1960s. Still, we also get the proper warnings against jumping to the quick but unwarranted comparative conclusions. Sannerstedt and Sjölin argue, for example, that in Sweden the "image of consensus is less salient, although political negotiations continue to be of great importance" (p. 99).

In his summary, Damgaard points to three substantial changes: weaker governments, increased parliamentary activity, and more parliamentary power. There is a decline of consensus but not a decline of parliament. We have, however, the Finnish exception—not, according to Dag Anckar, that Finland has gone through a decline in parliamentary power (though that belief is widespread) but that Finnish governments are increasingly majority governments. Due to legislative procedures that give one-third of the members the right to postpone and, in fact, "torpedo" almost any government proposal (p. 161), the threshold for a sustainable government majority in the Finnish parliament is 67%, not 51%. As Anckar points out, this gives the term "oversized majority" a quite different meaning in the Finnish case.

The Damgaard volume is highly recommended as an updated analysis of Nordic parliamentary trends. The country chapters are somewhat uneven, but all provide the kind of information useful for anyone aiming to keep a professional eye on developments in Nordic politics. The style and presentation would also make it suitable for courses on Nordic or West European politics. Some rather standard queries could, however, be raised about the book if viewed exclusively as a *comparative project*. The country presentations are bound together not by any coherent analytical frame but by a rather loose set of questions. The empirical material varies according to availability and author interests. Rommetvedt for example is particularly keen on the analysis of party dissents in the committee recommendations in the Norwegian Storting. More unnecessary, perhaps, is the individualistic approach to theories and conceptualization. Inevitably, therefore, the comparative discussion by Damgaard toward the end is nurtured mostly by the empirical qualities of the book.

Coalition Politics and Parliamentary Power is by Mats Sjölin, the coauthor of the Swedish chapter in the Damgaard volume. Its theme is the role of parliament and parties in political decision making. Empirically, it is a book about Sweden, but he also argues the theoretical case that the theory of political decision making could invigorate the study of parliaments and parliamentary practices in general. And although the decline-of-parliament thesis is much in focus in this book too, Sjölin's view is that this debate "is in need of *theorizing,* which applies to parliamentary research in general, and in need of *specification,* which applies to the question of decline in general" (p. 202).

The problem to be analyzed in Sjölin's book is "power relations and patterns of cooperation and conflict between the political parties in the struggle over the framing of policies" (p. 26). The empirical basis for this is the reports of the committees in the Swedish Riksdag regarding government proposals. The period covered is from 1970 to the early 1990s. Two empirical questions recur repeatedly throughout the book. First, how can we explain government–opposition processes in Sweden? Second, has the power of the Swedish parliament declined? It turns out that the opposition muscle is about as strong (or weak) in Sweden as in comparable Western democracies. Sweden is at least no deviant case in this respect. There has been a trend toward a higher level of changes in government proposals, enforced by the opposition, but equally striking is the ebb and flow in this relationship, connected with government base and party constellations in the Riksdag. There is no convincing evidence to substantiate the parliaments-in-decline thesis. In choosing between remaining alternatives—stable or increased parliamentary power—Sjölin argues that the ups and downs of parliament during the latter two decades leaves the issue unsettled. Still the weakening of governments may indicate a change toward *increased* parliament power. The question, however, is whether this derives from temporal fluctuations or lasting structural change.

Sjölin's eclectic approach in developing his theoretical argument makes refreshing reading. Here we also find his main mission, namely, to widen the field of traditional coalition analysis to include discussions about parliamentary decision making and coalition politics. The fatal fault of the comparative literature on government formation and parliamentary politics alike is the lack of trespassing—in other words, the insufficient amount of theoretical cross-fertilizing—within the tradition. Sjölin sets himself the task of bringing together various approaches at least in the sense of considering, within the same empirical analysis, the fruitfulness of coalition theory of the public choice tradition, the mainstream comparative government tradition, and "modern power theory."

Does he succeed? The aims are admirably ambitious, and Sjölin clearly has a point in attacking the compartmentalization of much research within this field. His discussions of the various approaches are fair and balanced. Still, there is not much new in, for example, his criticism of the formal coalition theory. At times, the presentation and discussion of the literature could have been shortened. The exercise takes a more fruitful turn when Sjölin comes to the bit in which he himself has more faith—the power theory, or tradition of power analysis. Employing concepts and distinctions like "autonomy" and the various "faces of power," he manages to put across the inadequacies in the debate on the decline of parliaments and also to present the complexities of parliamentary analysis in general. From the many shrewd observations on the exercise of power in parliamentary politics it also appears that Sjölin feels more comfortable among the foxes than among the hedgehogs in the community of scholars. The trouble with that position, however, is (as all foxes know) that it does not make the world easier to understand.

University of Oslo Knut Heidar

China's Quest for National Identity. Edited by Lowell Dittmer and Samuel S. Kim. Ithaca: Cornell University Press, 1993. 305p. $42.50 cloth, $16.95 paper.

This remarkably coherent symposium should command broad attention throughout our discipline because it illuminates in numerous ways the confusing complexities and uncertain relationships among such supposedly self-evident concepts as state, nation, country, culture, society, and civil society and such basic sentiments as nationalism, patriotism, and a variety of national, social, and political group identities. With embarrassing regularity, scholars will labor to define these concepts but then, in the pages that follow, slip into treating many as synonyms. The differences are important because political systems function quite differently depending upon the relative strength of each entity and the emotional attachments of their populations. Although this book is largely devoted to examining the Chinese case and although Chinese details, with the peculiar spelling of names, can be overwhelming for nonspecialists, there is enough food for thought in it to enrich the theoretical understanding of generalists, especially comparativists and theorists.

Indeed, the two editors in their joint introductory and concluding chapters explicitly address larger theoretical issues of national identity. In the process of relating the concept of national identity to political development theory, they argue that "the substantive content of national identity is the state and that the state defines itself by what roles it plays . . . in the international arena" (p. 17). Fortunately, however, they do not abide by such a narrow, state-oriented concept of national identity. (Possibly because the chapters are jointly authored, they are enlivened by a few harmless contradictions.)

The Chinese case—involving, as it does, a distinct civilization—is particularly helpful for highlighting many of the fundamental issues associated with national identity. The issue of whether the Middle Kingdom, a bureaucratic empire headed by emperors, should be classified as a nation–state highlights the problem of arriving at a proper definition of the nation–state. What does it mean that in modern times the Chinese "state" has undergone dramatic changes but the Chinese people have remained seemingly secure in their "Chineseness"? Is a powerful sense of cultural identity that does not involve the state a real form of nationalism? If these constitute valid questions, then maybe Michael Ng-Quinn is a bit too quick and easy in calling the ancient dynasties nation-states. Michael Hunt picks up the historical account with a review of China's reaction to the modern West and concludes that "the preoccupation with state power stood—and still stands—as one of the single features of Chinese national identity" (p. 76). His skillful recounting of the history does drift a bit from the question of China's "quest for national identity," so it is unclear whether the desire for a "strong state" reflects a weak sense of national identity or, rather, a sense of cultural identity that was so strong that the Chinese became hypersensitive to any sign of being inferior. Unquestionably the challenge of modernization forced many Chinese, like people in other non-Western societies, to question their traditions. In their confusion over why China was weak and the West seemed so powerful, Chinese intellectuals became obsessed with the idea that China needed a "strong state." Few Chinese intellectuals would have appreciated George Unwin's observation in *Studies in Economic History*: "The expansion of England in the seventeenth century was an expansion of society and not the State. Society expanded to escape from the pressure of the State." In contrast to the intellectuals, ordinary Chinese have generally known that things are always better, especially economically, when the political authorities are not overly strong. What neither the intellectuals nor the common people have ever understood is that China never developed an effective "civil society." It lacked a tradition of autonomous interests, engaged in articulating competing policy demands.

With respect to this deficiency, it is significant that Merle Goldman, Perry Link, and Su Wei start their interesting coauthored contribution on the current role of the intellectuals by noting a student's slogan at Tiananmen, We Love Our Country but We Hate Our Government, which showed that the students were able to distinguish between "state" and "society"—something that has historically eluded most Chinese intellectuals. The anthropologist James Watson clarifies a great deal by suggesting that in pinning down national identity, we should not necessarily focus so much on matters of beliefs but recognize that in some cultures identity is based more on valuing everyday practices, particularly ones with ritual overtones. That is, praxis counts for more than ideology. (Could it be that the Christian tradition has left secular Western scholars with an exaggerated sense of the importance of beliefs, so that we assume the durability of Communism in China depends upon whether, in their hearts, the Chinese really "believe" in the ideology when, in fact, it may be enough that they continue to *act* as though they believed?) In any case, an awareness of shared practices is apparently more important in defining Chinese national identity than questions of deep inner feelings and values. This view is in part confirmed by Richard Wilson, who reports on recent research findings that current Chinese socialization practices produce both continuities and changes in what it means to be Chinese.

The question of Chinese national identity is of growing political importance because it is widely assumed that the void created by the erosion of Marxist-Leninist-Maoist thought will be filled by Chinese "nationalism." But what is the content of that nationalism likely to be? The glib idea that economic growth can solve China's identity problem is challenged by Lynn White and Li Cheng, who show that the people in the economically dynamic coastal provinces continue to have identity problems. China's more active role in foreign affairs has also been of limited help, as Peter Van Ness demonstrates in his analysis of Beijing's attempt to identify with the Third World and Robert Scalapino in his recounting of China's relations with her neighbors.

In short, therefore, it is far from self-evident that the Chinese have a set of shared symbols, values, and myths that might be easily brought together to shape a Chinese self-image that could both guide domestic development and be projected in foreign relations. Elsewhere in the world, the end of the Cold War has forced us to reexamine the importance of various forms of *primordial group identities*. Good scholars that they are, the authors of this book have sought to build on the earlier work on political development and identity crises. (At the risk of seeming to be petty-minded, I must point out that there are some inaccuracies in their understanding of the contributions of the Social Science Research Council's Committee on Comparative Politics, but on the other hand it is good that they have recognized the significance for political analysis of Erik Erikson's theories.) What is important is that all who want to study the increasingly significant question of nationalism and national identities will have to build on this work.

Massachusetts Institute of Technology LUCIAN W. PYE

Making a Market: The Institutional Transformation of an African Society. By Jean Ensminger. New York: Cambridge University Press, 1992. 212p. $49.95.

Jean Ensminger has written an excellent book that addresses several different audiences. She is an anthropologist, but much of what she has to say is addressed to economists, sociologists, and political scientists. She "combines the individual-actor approach of economics, anthropology's appreciation of institutional constraints, incentives, and ideology, and the attention to power that we associate with Marxist analyses" (p. 1). The fact that the book cuts across so many different disciplines is one of its principal merits. The author draws from these different fields not ad hoc but in a systematic fashion. She argues that only through a synthesis of these different fields of study is it possible to obtain an effective understanding of how market forces contribute to social change. Her main objective is to show how rational choice and new institutional economic theories can be powerful analytical tools in explaining the process of change among the pastoralist Galole Orma of northeastern Kenya.

Ensminger argues that the best way to understand the process of change among the Orma is to go beyond the static analyses of classic economic and anthropological theories. She situates her analysis of the Orma within the analytical framework of Douglas North's *new institutional economic* perspective. The advantage of this perspective is that it assumes that institutions have an important effect on the options available to, and taken by, individual and social groups. Two key assumptions, in particular, inform the perspective. The first assumption is that transaction costs are not zero, so that change of any kind usually involves some costs. The second assumption is that economic exchanges are shaped by more than price and quantity: there is an underlying normative (qua ideological) dimension that cannot simply be dismissed on the grounds of being not quantifiable. For anthropologists and social scientists interested in socioeconomic and institutional change in the developing world, these two assumptions are important because they highlight the fact that in the short and medium term, economic change might generate more costs than benefits.

In her book, Ensminger sets out to explain how the spread of market forces transformed labor, social, economic, and property relations among the Galole Orma. She relies on historical and longitudinal data of a group of households to show the asymmetric effects that the penetration of market forces have had on the Galole Orma. By employing both the new institutional economic perspective and the ethnographic approach of anthropology, she is able to analyze the process of institutional and ideological change within the Galole Orma community. In particular, she shows how these changes gradually led to a shift within the Galole Orma community away from pastoralism toward a more sedentary community of cattle herders.

Her application of the new institutional economic perspective to the study of the process of change among the Galole Orma is generally convincing. For political scientists, the most striking claim made by the author is the one made in chapter 6. There she argues that "in the aftermath of increasing economic growth and resultant economic diversification among the Orma, there was a breakdown in community and a failure of collective action" (p. 143). While she is quite persuasive in her argument as to why there was a breakdown (particularly because of the increasing inability of elders to enforce property rights), she overdoes her case when she claims that the failure of collective action at the community level was effectively supplanted by the state in Kenya. Her attempt to explain state formation within the context of new institutional economic theory becomes dangerously mechanistic at times. Her argument that the benefits offered by the state through its increased institutional penetration of peripheral communities such as the Orma have led to growing acceptance of the state assumes a rational outcome that is difficult to sustain in Africa today.

The fact that the penetration of market forces and state institutions have had a significant impact on pastoralists like the Galole Orma is undeniable. To make the claim, however, that "the lessons from this case study of state incorporation speak also to the issue of state formation, and more generally to the relationship between economic growth and the breakdown of communities, and the institutions that support communities, in this case marriage, lineage, clan, gerontocracy, patriarchy, and patron–client relations" (p. 143) suggests that the author does assume that change always moves in the direction of increasing efficiency or economic growth. Although she recognizes that there have been losers, particularly the nomads, she implies that there has been a convergence of interests between the state elite and Orma elite and nonelite alike.

The main point is not that the penetration of market forces and state institutions has not reshaped the Orma community and led to a breakdown of traditional patterns of collective action. What is at issue here is that this process of change in Africa rarely, if ever, involves the complete breakdown of "lineage, clan, gerontocracy, and patron–clinet relations." In most instances, these elements continue to influence institutional change and have an impact on economic development. Whether elites use these quasi-traditional elements to enhance their own political and economic positions, or the elements persist because institutional and economic change is "lumpy" and reflects multiple asymmetries, the outcome in Africa, at least in terms of institutional and economic development, has generally been a suboptimal one.

Purdue University Dwayne Woods

The Politics of Food in Mexico: State Power and Social Mobilization. By Jonathan Fox. Ithaca: Cornell University Press, 1993. 280p. $39.95.

The sun has not set on hopes that state bureaucracies in developing countries can implement reforms biased in favor of the poor. This hope persists despite the relatively sad record to date. The literature is replete with examples of reforms that have been stillborn or sabotaged or have produced unintended and even damaging consequences. Constraints on reformers with grand objectives are considerable. They must convince political leadership to back their change strategy. They must devise the policy with little information on how it will fare in practice and show quick results to maintain momentum. They must be highly skilled in managing the administrative, political, economic, and ideological dimensions of the new policies until they are self-sustaining. Reform policies, however, raise a red flag to supporters of the status quo. The policies divert resources from existing programs, whose committed supporters in the bureaucracy and civil society oppose and undermine the changes. The poor are typically unorganized and offer few resources to support the reformers other than a promise to accord legitimacy to the regime if the reforms work. Economic cycles inevitably squeeze the government treasury, and political leadership changes—resulting in a review, and sometimes abandonment, of policy just when the new program might be showing accomplishments.

Jonathan Fox confronts each of these issues head-on in a theoretically relevant, well-documented, and sensitive analysis of food policy in Mexico. His case study is the Sistema Alimentario Mexicano (SAM), a rural-development-cum-food-distribution policy instituted under Mexico's Lopez Portillo presidency (1976–82). In 1980, the Mexican government was alarmed by rapidly rising food imports but was not about to scrap the 1917 peasant landholding legislation, which, through the sacrosanct collective farm unit (the *ejido*) suboptimized domestic food production. José Lopez Portillo gave status and backing to a young team of agrarian reformers who had new ideas: make peasant producers the prime beneficiaries of state subsidies in seeds, fertilizers, credit, technical assistance, grain purchase, and water, while extending the state food distribution network to remote rural areas. the goals were to increase production, save foreign currency, reaffirm the viability of the peasant farmer, raise nutritional levels, and enhance political legitimacy for the regime from groups who, seven decades before, had been the footsoldiers for the Mexican Revolution.

The reformers ran into typical problems: opposition from agricultural bureaucrats who wanted all subsidies to go to large farms in irrigated areas; decline in high-level backing as Lopez Portillo became a lame-duck president; a state-centered political system in which peasant organization was inherently weak; and sharply reduced state finance after the 1982 debt crisis. Fox traces the orientations and capabilities of all the major actors in this drama (e.g., DICONSA, a food distribution agency) and concludes that the policy was most effective where reformers succeeded in organizing the peasant beneficiaries in the context of the reform itself. For example, newly created community food councils used official regulations, shrewdly drafted by the reformers, to hold DICONSA agents accountable for delivering food to rural stores where peasants could buy at subsidized prices.

This research forms part of the literature voguishly called the "new institutionalism." Fox eschews the label and deals industriously with the timeless concerns of organizational effectiveness and responsiveness. The book quite adequately explains the position of the major players in the complex Mexican political system, which helps show why the reform took the course it did. Fox dismisses what he sees as a dichotomy between state-centered and society-centered analyses of bureaucratic behavior and posits the *interactive* alternative, which combines the two. An example of an interactive finding would be change instituted by elites which impinges on beneficiaries, who constitute themselves into groups, which ally with the elites to create a reform coalition.

This analytical concept is linked to a suggestive strategy for reform mongering. The "sandwich strategy" is not a food handout but, rather, an objective alliance between entrepreneurial reformists and autonomous social movements to offset the power of entrenched authoritarian elites. The "sandwich strategy" is the SAM reformers' contribution to praxis, skillfully articulated by Fox, who quite rightly claims that the strategy has cross-national implications. Scholars and practitioners alike are well advised to study Fox's findings in this realm for theoretical and practical guidance, and chapter 7 (which studies the technique) would fit well into any course on policy formulation and implementation.

One criticism is that Fox neglects to discuss how food policy and peasant agriculture were affected by subsequent events. After the Lopez Portillo government, agrarian reformers vainly waited for a "Son of SAM" to rescue the collective-landholding ideals of the Mexican Revolution while allowing Mexico to feed itself. Under Miguel de la Madrid (1982–88), the Mexican state was penniless and ignored the peasant farmer. Carlos Salinas de Gortari (1988–94) then amended the 1917 Constitution in a way that many analysts believe will end the *ejido* and permanently change the context for official policies toward the rural poor. The value of Fox's contribution is thus not his description of the SAM but the expertness with which he uses Mexican food policy to draw important theoretical and practical lessons for state-induced change. The findings, which emphasize targeted organization of the poor more than class mobilization, are salient in one degree or another for Asian, African, and other Latin American cases, as well as for traditionally neglected groups in the United States and other richer countries. *The Politics of Food in Mexico* is a fine example of a case study grounded in exhaustive field research. It addresses real world problems in a theoretically innovative way.

University of Texas, Austin PETER S. CLEAVES

Nations of Immigrants: Australia, the United States, and International Migration. Edited by Gary P. Freeman and James Jupp. New York: Oxford University Press, 1993, 250p. $18.95 paper.

Is the process of state-formation among "nations of immigrants" distinctive in some important ways? Strange as it seems, despite the considerable weight attributed to immigration in explaining the political, economic, and social development of the United States and the small number of other countries that fall within this category, little attention has been devoted to the more general question. And perhaps even more surprisingly, historians and social scientists concerned with particular countries within the category have seldom availed themselves of the possibility of comparison to test their observations, or at least enrich them by engaging in the sort of counterfactualism that is rendered possible by relatively similar experiences elsewhere. As the editors of the present work remark in their introductory essay, "One of the very few serious attempts to understand settler societies in a comparative framework is Louis Hartz's *The Founding of New Societies,*" an edited volume published thirty years ago, when "political culture" reigned unchallenged (p. 9). But although the Hartz collection contains some stimulating essays, surely even the most conservation-minded among us would agree the field has remained fallow long enough to stand vigorous cultivation.

Although less ambitious, the present volume is a well-executed collaborative examination of the sources and consequences of recent developments in immigration policy in the United States and Australia by scholars from both countries. It is a stimulating beginning in a promising area of research, and contains valuable suggestions for more comprehensive projects along the lines noted.

Comparison, here, serves not so much to achieve broad generalizations as to provide a better understanding of each case by highlighting its specificity. The introductory chapter by the editors sketches the distinctive historical configurations of the two countries, highlighting differences as well as similarities. The juxtaposition of the two cases brings to mind that the notion of "nation of immigrants" should be made time-specific, reflecting a combination of objective criteria (e.g., the rate of immigration in relation to population) and subjective perceptions. In this perspective, the United States was *not* a "nation of immigrants" at the time of the Founding, but became one from about 1830 until it acted very effectively to change its character in the 1920s; Australia in effect *foreclosed* its transformation into a "nation of immigrants" by closing its borders in the 1890s, before the arrival of a significant flow of continental Europeans and Asians. Both countries reopened in the late 1960s; but although Australia is today much more a "nation of immigrants" than the United States (about one out of five Australians was born abroad, as against only one out of twenty Americans), immigration into the United States has been and remains a source of much greater ethnic diversity. Despite the broadening of sources of immigration in the post-war period—first to encompass southern and eastern Europeans, and later Asians—as of 1986, 75% of the Australian population was of British or Irish descent; and immigrants of "English-speaking background" still account for nearly half of the annual intake today.

The chapter also provides some intriguing theoretical suggestions regarding the role of immigration in shaping the very different patterns of working-class formation in the two countries. Overall, Australia emerges as a "might have been" California—if the transcontinental railroad had not been built—i.e., an isolated white labor commonwealth that managed to maintain control over membership until the middle of the twentieth century, and thereby shape a distinctive path for capitalism and state-formation. The chapter might have been further enriched with some maps, tables, and graphs with basic information regarding historical and contemporary patterns.

Part I provides detailed accounts of immigration policy proper as well as of its underlying political dynamics. Although uninformed American readers will find the chapter by Robert Birrell on Australian policy especially useful, Frank Bean and Michael Fix also provide a very good summary of recent developments on the American side. The comparative analysis by Freeman and Katherine Betts of "the politics of interests in immigration policymaking" is of greater theoretical import. They conclude that immigration generates conflicts between the same sorts of interests in both countries, but their relative weight varies. In keeping with the distinct historical patterns noted, labor remains more influential in Australia; and despite the fact that ethnic groups have been given a corporatist status in the Australian system, they have greater legitimacy as political actors in the United States.

Part II is devoted to the impact of immigration on the economy; reviewing the recent macro- and microeconomic literature, its two chapters concur that, notwithstanding significant differences in the skill level of immigrants, in both countries immigration has had a "Pareto optimal" effect, i.e., it has increased the welfare of the immigrants without generally reducing the economic position of most residents, while substantially benefiting many of them.

Part III, on "Immigrant Settlement," highlights one of the sources of persistent differences between the two countries. From the very outset until quite recently, immigration to Australia was sponsored and funded in whole or in part by government, whereas on the American side (with the major exception of African slaves) "immigration begat immigration," by way of the networks generated by earlier arrivals. Concomitantly, Australian authorities exercised much greater control over the choice of immigrants, and played a more active role in the process of settlement as well, whereas on the American side the immigrants were largely left to their own devices after arrival. By and large this is still the case today in both countries. Consequently, there is an ongoing national debate in Australia regarding *the* appropriate policy of incorporation, whereas on the American side the subject is much more diffuse. While the Australian situation is well presented by James Jupp, the American chapter by Robert Bach is more problematic. In part, as Bach points out, the very structure of the situation makes it difficult to talk about American "settlement policies"; nevertheless, more attention might have been given on the American side to refugees—currently, some 130,000 a year—whose settlement involves legally prescribed intervention by all levels of government, and to the extensive activities of states and especially urban governments in this sphere.

Last but not least, Part IV is addressed to what constitutes throughout the world of industrial democracies the hottest aspect of immigration—the matter of incorporation of culturally distinct groups. Although entitled "Immigrant Minorities and Multicultural Policies," the contribution by Stephen S. Castles, one of the most valuable in the book, amounts to a deconstruction of the doctrine of "multiculturalism," which was launched in 1989 as the official policy of Australia. Contrary to what the term evokes in the American context (and thereby in Europe as well), in Australia multiculturalism "is not defined in terms of cultural pluralism or minority rights, but in terms of the cultural, social and economic rights of all citizens in a democratic state" (p. 190). As presented by Castles, most of the measures under this umbrella are designed to reduce various forms of institutional discrimination non-Anglo immigrants, old and new, face in egalitarian Australia. In short, *this* "multiculturalism" is highly universalistic in its import, and when applied to immigration policy means that Australia will welcome highly skilled immigrants even if they are Asian. Beyond this, the adoption of multiculturalism as a general policy also entails limited government support for ethnic organizations and ethnic media, features that are highly developed in the United States, possibly because of the longer experience of minority advocacy where a critical mass was present because of sheer size (e.g., the organizational flowering of Jewish and Catholic minorities around the turn of the twentieth century) and the concomitant dynamics of the market.

Although DeSipio and de la Garza provide some suggestive observations regarding incorporation on the American side, particularly on the subject of access to citizenship (which is not covered with regard to Australia), their essay unfortunately does not provide the materials for a systematic comparison with the Australian situation as presented by Castles. Given the acute public concern with incorporation in all contemporary receiving societies, and the conceptual confusion in the proliferating literature on the subject, a systematic comparative analysis of the situation should rank high on our research agenda.

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Environmental Risk, Environmental Values, and Political Choices: Beyond Efficiency Trade-offs in Public Policy Analysis. Edited by John Martin Gillroy. Boulder: Westview, 1993. 189p. $42.00 paper.

Contributors to the Gillroy volume argue that when traditional market methods, alone, are used to analyze public choices, conflicts over environmental risks often resist resolution. Including original essays by chemists, economists, philosophers, and political scientists, the collection is divided into three sections. Parts One and Two—with essays by Gillroy, Steven Kelman, Robert Paehlke, Vicente Medina, Christopher Bosso, and David Henderson—analyze the nature of environmental values and risks and provide suggestions for handling conflicts concerning them. Part Three, with essays by William Gunderson, Talbot Page, Barry Rabe and Gillroy, and Douglas MacLean, surveys several cases of citizens' NIMBY (Not in My Back Yard) responses to attempts at siting environmentally risky facilities.

Because the Gillroy collection encompasses articles exhibiting a wide range of disciplinary, methodological, and ethical standpoints, there is no specific thesis for which all the essays argue. Nevertheless the excellent introductory essay (chapter 1) by Gillroy provides two general themes addressed by all the contributors. One theme is the complicated nature of environmental risks and values, complications caused in part by the collective nature of many environmental goods. The second theme is the necessity of incorporating environmental values into our analyses of public decisions regarding risk. Although virtually all of the authors do an excellent job of defending and articulating the first theme, Page, Rabe and Gillroy have the best specific suggestions regarding the second theme, how to incorporate environmental values into policymaking. This incorporation is difficult, Gillroy argues, because economic methods, such as cost-benefit and risk-benefit analyses, help to construe the preferences of individuals in terms of a polluter's (prisoner's) dilemma. The dilemma is that, given the strategic reality of interpersonal relations, individual preferences result in a non-optimal collective provision of environmental quality. Incorporating environmental values into policymaking is also difficult, for example, because federal regulatory agencies fight each other in environmental turf wars (p. 82); because the Environmental Protection Agency has little clout (p. 86); because the American political system devalues planning and activism (pp. 90–91); and because the public assesses environmental risk in an unscientific way (p. 106).

Showing how to overcome such obstacles to including environmental values in public decisionmaking, Rabe and Gillroy use a Canadian case study. The case, on siting a hazardous waste facility, provides an illustration of a new strategy for environmental decisionmaking. Instead of modeling such decisions on the prisoner's dilemma, Rabe and Gillroy propose an assurance-game model in which universal cooperation, rather than being a free rider, is the preferred choice (pp. 156–157).

Obviously Rabe and Gillroy theoretically are correct about the desirability of the assurance-game model. The question their analysis raises is how to motivate the players in an environmental game to operate in terms of cooperation strategies. Hence the real question their analysis raises is not whether assurance-game models are desirable, because obviously they are, but whether real situations of environmental conflict can be resolved by following such models. Gillroy admits that using the assurance game—to model decisionmaking regarding environmental risk in NIMBY situations—requires full information, no uncertainty, and no suspicion on the part of any persons involved in the NIMBY situation (p. 166). Because such idealized conditions are rarely met in problematic controversies, and because risk imposers typically withhold information from the public, it is difficult to believe that the conditions necessary for using the assurance-game model are applicable to most NIMBY controversies. Moreover, it is nothing new that analytic models often fail to solve real-world problems. As Gillroy recognizes, cost-benefit analysis falls short in environmental problem solving. What we require now are precise and empirical arguments that show assurance-model strategies are more believable and more realistic than cost-benefit models. For real situations to follow the assurance model, we need evidence about the possibility of motivating persons to behave in cooperative ways. If Rabe's and Gillroy's Canadian case study about siting a hazardous waste facility is representative, then their conclusions about assurance-game models are correct. If their single case study is not representative, then their proposed model may be less promising than they believe. Regardless of which is the case, the book provides us with a heuristically powerful, concrete solution that can be attempted and tested in future conflicts over environmental risk. Moreover, apart from whether the assurance-game model will work in most cases of environmental conflict, Gillroy has done a great service by pointing out that successful models of conflict resolution (like the proposed assurance model) presuppose full disclosure of relevant risk information, unquestionable integrity on the part of all parties to the controversy, and great public trust. Whether or not the assurance model works, in clarifying it, Gillroy has done a brilliant job of exposing the most potent reasons for continuing environmental conflict and for lack of cooperation among parties. If he is correct, the conflicts come not so much from poor or incomplete science but from withholding information and lack of credibility and trust. He has produced a compelling argument for examining political and ethical, rather than merely scientific, components of environmental risk.

As a policy analyst, Gillroy provides a persuasive and clear dissection of collective-goods problems. His essays are among the best in the volume, not only because of the quality of the analysis but also because of their comprehensive survey of types of environmental values and the complexities of environmental risk. This particular asset of the volume makes it useful as a text. Yet another strength of the collection is its insightful and thoughtful treatment of some of the flawed market mechanisms for dealing with environmental risk.

Apart from questions about the real-world applicability of the assurance model, the main shortcoming of the book is that, despite the unity of the Gillroy essays, many of the other contributions in the collection are not tightly connected to each other. Although there are two general themes running throughout all the chapters, the diverse contributions to the book have no common, specific focus. For example, the Henderson essay, on flawed public understanding of science, is outside the general theme of the volume, as is the MacLean essay on conservation versus preservation. There is also some redundancy in the collection, particularly in the discussions of prisoner's dilemmas. Nevertheless, the excellent analyses by Gillroy and the volume's inclusion of original essays by some of the best persons working in the field—Kelman and Page, for example—make it an important book for anyone interested in environmental risks, political choices, and game-theoretic strategies.

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KRISTIN S. SHRADER-FRECHETTE

Politics, Society, and Democracy: The Case of Spain. Edited by Richard Gunther. Boulder: Westview, 1993. 272p. $44.00.

The Return of Civil Society: The Emergence of Democratic Spain. By Victor M. Pérez-Díaz. Cambridge: Harvard University Press, 1993. 357p. $35.00.

The two books under review here are quite different in scope, but they share a common interest in exploring the recent history of Spain and the reasons for its successful democratization. *The Return of Civil Society*, by Victor Pérez-Díaz, is a book that can only be described as magisterial. It moves easily and successfully along the boundaries of history, political science, and sociology, presenting a carefully crafted argument about the democratization of Spain while reflecting intelligently and provocatively on many other subjects. Among these, the conditions for democratic development; the nature of the relationship between social institutions, market, and state; the changing nature of religiosity and the adaptive capacities of religious institutions; and the impact of neocorporatist or territorial arrangements (he refers to these as *mesogovernments*) on the efficacy of national governments.

Politics, Society, and Democracy makes a different kind of contribution to the literature. Ably edited by Richard Gunther and part of a four-volume festschrift in honor of Juan Linz, this book focuses on Linz's contributions to the study of Spain. Its chapters, written by former students and colleagues, testify to the breadth and intellectual rigor of his work, his collegial spirit, and his capacity (as José Cazorla puts it) to employ Spain as a "paradigmatic" case (p. 71). Although not entirely immune to the problems associated with edited volumes, it is an important book and helps us better understand not only the work of Juan Linz but also important aspects of the Spanish experience in the twentieth century.

Politics, Society, and Democracy is part intellectual biography, part political analysis. In part 1, Amando de Miguel paints an affectionate portrait of Linz, while Manuel Gómez-Reino and two others from DATA, S.A. (the social science survey research firm that Linz helped found) show the link between Linz the citizen and Linz the social scientist, between Linz's commitment to democracy in Spain and his use of Spain as a prism through which "to better comprehend the most important problems of the contemporary world" (p. 11). Miguel Jerez takes up a third dimension in the life of Juan Linz—his role in the development of the social sciences in Spain.

Part 2 focuses more directly on themes related to Spanish politics and society. Among the topics covered in its 11 chapters are a comparison of the labor movements in Spain and Italy (Malefakis), authoritarianism and its legacy (Cazorla), the legitimacy and meaning of democracy (Montero), the role King Juan Carlos played in the democratization process (Polodny), the role of business (Martínez) and labor (Fishman) elites, the evolution of the Basque party system (Llera), and health policy reform under the Socialists (de Miguel and Rodríguez). I should like to draw attention to those chapters whose themes relate closely to the issues discussed in the *The Emergence of Civil Society* by Victor Pérez-Díaz, namely, the role of societal actors and the cultural foundations of democracy.

The role of societal elites in the construction of Spanish democracy is the subject of the chapters by Martínez and Fishman. The Martínez chapter demonstrates the positive role that the business community played in the democratization process. His conclusions clearly support the argument that the diffusion of economic control is an important stimulus to democratic development. (My only quibble with this otherwise excellent chapter is that Martínez fails to analyze in much detail the political role of the banking sector. He is right to call the *gran banca* "more a polemical than an analytical concept" [p. 118], but one does not need to have a particularly conspiratorial bent to assume it had an active hand in the disintegration of the UCD.)

Fishman covers the other side of the societal–industrial divide. His chapter is an excellent example of how to explore the differential impact that subnational differences and variations in social structure had on working-class radicalism in Madrid and Barcelona. More relevant to our discussion, however, he demonstrates why radical working-class elites in Barcelona failed to retain their influence among a working class that was more moderate.

Cazorla and Montero touch on issues related to the cultural foundations of democracy. The chapter by Cazorla ("The Theory and Reality of the Authoritarian Regime, Thirty Years Later") is extraordinarily suggestive and useful. Not only does it discuss some of the debates surrounding the concept of authoritarianism, but it also explores the "residues" of *franquismo*, specifically the lingering distrust of politicians and the emergence of what Cazorla terms "mass-level clientelism" and the "collective patron" (p. 83). His conclusion is that "the rapidity of our political change has produced an asynchronism—the survival of the remains of an authoritarian mentality within a minority but visible sector of the population, combined with the inertia of important institutions and improvisation or a lack of scruples by some of their leaders" (p. 87).

By contrast, Montero offers a rather more optimistic assessment of the situation. Focusing on legitimacy and efficacy (two central concerns in the work of Linz), Montero analyzes the so-called *desencanto* of the late 1970s and early 1980s. He argues that despite lingering positive feelings about Franco (29% of respondents were "sympathetic" toward him in a 1982 survey), the legitimacy of the democratic regime was never in question. Among the chief reasons for this, Montero explains, were that Spaniards, having recently experienced a transition from authoritarian rule to democracy, could make quite "separate evaluations of system performance (efficacy) from support for the current democratic regime (legitimacy)" (p. 155). The outcome of the crisis of the early 1980s may have been partisan dealignment (or, as Samuel Barnes, Peter McDonough, and Antonio López Pina put it several years ago, "volatile parties and stable voters"); but there was (and has been) very little support for antidemocratic solutions or antisystem parties. Montero also advances a second line of argument, insisting there has been a "displacement of the traditional dichotomy liberty–equality by (a) new materialist–postmaterialist cleavage" (p. 166). There is certainly empirical evidence in this regard, but I think Montero might not stress the connections between these "new" values and the "residual" ones inherited from *franquismo* enough.

The debate over civil society and the cultural foundations of democracy are at the core of contemporary political science debates. And it is in this area that the other book under review here makes its most important contribution. *The Return of Civil Society* offers an absorbing *alternative* reading of how and why democracy came to Spain. The heroes of this tale are not the usual cast of the political elites or *clase política*. "Political transitions," Pérez-Díaz cautions, "can be reconstructed *ex post facto* as critical choices by the elites, but most often those choices are not so much deliberate calculations as belated reactions to ongoing processes the elites hardly understand, must less control" (p. 7). Indeed, when he reviews critical events (such as those of 1976–77), Pérez-

Díaz emphasizes not so much what the elite did but how the public responded. The new political rules, he argues, were "not so much invented *de novo* as translated, so to speak, from (the) meta-rules of social, economic and cultural life" (p. 28).

For Pérez-Díaz, the "emergence of democratic Spain" is about the "return of civil society," that is, the development of "social institutions such as markets and voluntary associations and a public sphere which are outside the direct control . . . of the state" (p. 57). It was in the civil society, with the trauma of the Civil War and the experience of Francoism as primary points of reference, that a democratic tradition was invented, whence it was eventually transported to the political sphere.

In seeking to explain the emergence of democracy in Spain, Pérez-Díaz finds insufficient those arguments that center either on structural variables (be they economic or cultural, domestic or international) or on elite choices—not that he dismisses those variables, for the reader will find a sympathetic and knowledgeable discussion of their explanatory strengths and weaknesses. (Particularly notable is his discussion of the impact that international variables had on the democratization process. Those who are familiar with the transition literature know how often this factor is ignored.) Rather than dismiss structural or elite variables, Pérez-Díaz proposes to shift their terms of reference; to approach structural factors in terms of the traditions, rules, and institutions at their core; and to view actors "less as choice makers than as rule followers (or as rule deviants or rule breakers)" (p. 26).

It is from this perspective that Pérez-Díaz examines the role of societal groups and institutions (business, labor, and the Church) in the "emergence of democracy" in Spain. His conclusion is worth quoting at length:

> The effect of businessmen and trade unions has been greater and more constructive than the rather more modest influence exercised by intellectuals. The image politicians like to put forward of themselves as holding together the world which is breaking apart seems plausible when one compares them with the cultural groups, but much less so if they are compared with socioeconomic groups. The market and industrial development have been important binding forces throughout the Spanish territory for the last century and a half. Business and trade unions have been powerful centripetal forces, tending toward the integration of the national community (p. 209).

The Return of Civil Society is more than just about why democracy "emerged" in Spain. It is also about examining the challenges that democracy confronts (there and elsewhere). From this perspective, Pérez-Díaz explores two sets of issues: (1) the nature of *mesogovernments* (neocorporatist arrangements and territorial formulas of various types) and the "problematic effects" these structures may have on governability and on the economy and (2) how the nature of the relationship between workers and unions has changed, especially with the breakdown of the post-1945 "social compromise" and the growth of a "rational, instrumental, or experimental attitude" toward unions and parties (pp. 193, 284, 298 and chap. 4). Addressing this last theme from the same "societal" perspective he employed in discussing the "emergence" of democracy, Pérez-Díaz argues that given the "moral and cognitive development" of the workers, "unions and political parties could not but (have) followed a strategy of *de facto* accommodation with capitalism" over the past several decades (pp. 311–12).

The Return of Civil Society is an eloquent call for "bringing society back in" to the study of comparative politics. Although not necessarily agreeing with everything Pérez-Díaz says, the reader will gain many insights from it. My only complaint pertains to his concluding chapter. I wish he had connected his earlier discussions of clientelism and political corruption more clearly with his later argument about workers, unions, and the social pact. This I say primarily because I wanted Pérez-Díaz to expand on this issue and its relation to governability. Pérez-Díaz has written a superb book that will make a significant contribution to the ongoing debate about the process of democratization.

Georgetown University EUSEBIO MUJAL-LEON

Apple of Gold: Constitutionalism in Israel and the United States. By Gary Jeffrey Jacobsohn. Princeton: Princeton University Press, 1993. 284p. $39.50.

Once dismissed as an artifact of eighteenth-century Europe, constitutionalism seems to be enjoying a certain renaissance. New or revised constitutional texts, inescapably accompanied by debate over more broadly constitutive matters, now figure prominently in dramatic political transformations nearly everywhere—all over the post-communist region, in South Africa, in Israel and Palestine, in India and in Canada. Since this new round of constitutional politics follows on the heels of the recent bicentennial of the American constitutional text and Bill of Rights, it is not surprising that constitutional scholarship is blossoming. Gary J. Jacobsohn's book is among the most astute and far-seeing of the current batch of books on comparative constitutional issues. In fact, it is hard to overstate the importance of his book because its significance transcends its immediate subject. He compares Israel and the United States, but his comparison speaks powerfully to the identity crisis of liberalism in a multicultural era.

Liberalism has been entwined with constitutionalism ever since they emerged in the eighteenth century as apparently fraternal twins. So much have their careers been linked that the term "liberal constitutionalism" is often taken to contain a superfluous adjective. But while constitutionalism—somehow "the rule of law" as against "the rule of men"—is riding high, liberalism is under siege. Non-western intellectuals join western postmodernists to attack the notion of individual autonomy rights as a form of naive universalism, and to unmask the ethnic and moral "neutrality" of the liberal state as a covert form of coercion. And many states now face new demands for group rights and for state patronage of religious or ethnic identities. These demands arise out of increasingly multicultural conditions everywhere, suggesting that the very social pluralism which was supposed to necessitate liberalism in the first place now threatens to strangle liberalism with its own equality principle. In the American heartland of liberalism, for example, the crisis is visible in the Kiryas Joel case now before the U.S. Supreme Court. The case involves a challenge to a public school district specifically created to provide handicapped education to a Hasidic Jewish community in New York state. It forces the Clinton Justice Department (which must weigh in before the

Court) to choose between two parts of its own soul—between liberal multiculturalism on the one hand and liberal religious neutralism on the other.

With Jacobsohn's help we see how vastly different such questions appear when considered from the perspective of Israeli constitutionalism, which he calls an "alternative pluralism." In the U.S., pluralism is said to require the state (and its schools) to be scrupulously neutral among rival sub-communities and their values. In Israel, pluralism requires the state to accommodate—and empower—the cultural institutions of various religious sub-communities, even while it publicly affirms the centrally Jewish identity of the polity as a whole. The American rule of religious neutralism, if transplanted unmodified, would shatter the identity of the Jewish state, and would be regarded by most Israelis (Jew and Arab alike) as a homogenizing doctrine corrosive of the actually existing plurality of communities. Americans may find such thinking incongruous. But in a brilliant move of internal comparison, Jacobsohn points them to the one "glaring exception" to America's liberal constitutionalist norms: the native American tribes (p. 18). Based on early treaties and their claim to primeval nationhood, the tribes, as communities, enjoy constitutional recognition in a manner more consistent with the Israeli than the American brand of constitutionalism. Most revealing was the 1968 "Indian Civil Rights Act," opposed by native Americans as an assimilationist instrument whose exaltation of individual rights would erode tribal cohesion. Jacobsohn notes that the final version of the Act pointedly omitted any no-establishment-of-religion clause, since the integration of spirituality and public order often constitutes tribal identity. As Jacobsohn says of Israel, such communal autonomy "supports political stability by providing non-dominant (and inassimilable) groups with mechanisms that enable them to minimize the effects of their inferior position in the larger society" (p. 35).

Liberal doubters might dismiss the native American case as a constitutional regime's pragmatic compromise with a marginal anti-constitutionalist sub-community. The bigger question, then, is whether such arrangements qualify as a version of constitutionalism when, as in Israel, they mark the scheme as a whole. Does the Israeli experience represent its own principled form of constitutionalism, as Jacobsohn seems to hold, or does it qualify only to the extent that it conforms to the norms of American-style individualist liberalism, with deviations regarded as anti-constitutionalist malfeasance?

The State of Israel does not, of course, have a formalized constitutional text; it has an incomplete set of "Basic Laws" on different subjects passed by the Knesset, and no list of constitutionally protected individual rights. In the absence of a definitive document, the 1948 Declaration of Independence has loomed large. Along with the 1950 Law of Return, the Declaration continues to define the Israeli policy, and under its terms the Israeli Supreme Court has developed a practice of judicial review that includes substantial protection for individual civil liberties. Score one for Israel, at least by American criteria.

Yet Jacobsohn does not gloss over the lack of a text. Creating a unified text is a daunting task, he explains, in the face of the tensions which comprise the Israeli polity. For Israel is constituted by its dual commitments—announced in the Declaration—to being a Jewish state *and* to ensuring equal rights and freedoms for "all its inhabitants." Jacobsohn calls this "a mix of universal and particularistic principles" (p. 237). In several rich chapters, he examines the political and jurisprudential consequences of this mixture. He examines the recent controversy over amendments to the Law of Return (Who is a Jew?). He shows how the Israeli Court's dominance has been curtailed by the mix of constitutional principles just as much as by the tradition of parliamentary supremacy. He reviews education policy, national security censorship, and free-speech jurisprudence, most intriguingly the Kahane case and the ban on the showing of the film *The Last Temptation of Christ* (banned briefly out of respect for the sensibilities of Israel's minority Christian community).

Jacobsohn shows how ambiguity characterizes Israeli jurisprudence in all of these areas. He is far from portraying Israeli law as consistently communitarian or illiberal; by his own account the outcomes often match those of American courts. But with careful attention to nuance, he demonstrates how, even in those cases which seem to track most closely with American jurisprudence, there remains among Israeli judges a principled reticence to embrace the liberal view wholesale, and a consistent recognition that Israel's Jewish identity, and its fragile cultural context, make it unwise—even unconstitutional—to replicate the jurisprudence of individual autonomy rights.

In his final chapter, Jacobsohn broods on the theoretical implications of his comparison. He struggles to articulate the meaning of constitutionalism independent of liberal criteria. As illuminated by the Israeli experience, constitutionalism "is definitely not *illiberal* politics," he says, "but its conceptualization should not be confined only to polities that embody an unambiguous, robust commitment to the ends of liberal democracy" (p. 237). I think his own remarkable argument suggests that we go further still. He makes me wonder whether a mixed constitution of the Israeli variety isn't in fact more authentically constitutionalist than *any* of the unmixed varieties which (like "liberal constitutionalism") do not obstruct the despotism of their own principles.

University of Pennsylvania GRAHAM WALKER

Decision Making in the European Community: The Council Presidency and European Integration. By Emil J. Kirchner. New York: St. Martin's, 1992. 165p. $24.95 paper.

The entry into force of the Maastricht Treaty on European Union on 1 November, 1993 came after much delay, including a razor-thin referendum vote in France and two such votes (the second was to deal with an initial rejection) in Denmark. The treaty has introduced a new level of complexity (some would prefer to characterize it as a new level of confusion) into the nomenclature, decision-making process, institutional framework, and relations between Brussels and the national governments of the 12 European Community member states.

The European *Union* is now the umbrella term referring to the already-existing "three-pillar" structure of the European Community (the European Coal and Steel Community, European Economic Community, and European Atomic Energy Community) and the two new "pillars" constructed by the Maastricht Treaty: the Common Foreign and Security Policy enhancing the new European Political Cooperation process and certain areas within the domains of the justice and interior ministries (especially greater collaboration/cooperation between police and other authorities on crime, terrorism, and asylum–immigration issues). The European *Community* still continues to exist as a separate legal entity within the broader framework of the Union; but, in view of the difficulties of delineating what is strictly EC or Union business, the term of choice is now the European Union, rather than the more limited Community, as contained in the title of this book.

Compounding this complexity, there exist two Councils: (1) the Council of Ministers of the European Union (formerly "of the European Communities") comprising (usually but not exclusively) the foreign affairs ministers of the 12 member states and (2) the European Council, comprising the heads of state or government of the member states. The "ministerial" Council meets quite regularly (about 80–90 times a year), whereas the "prime-ministerial" Council meets at a biannual summit. Each Council has its "Presidency"—a rotating (six-month term) chief presidency officer or "President-in-Office."

At the level of the European Council, the head of state or government of the member state holding this rotating six-month office acts as host, chairs the meeting, sets the agenda, acts as official spokesperson, and (depending upon a series of conditions) may be able to set the tone and nature of Union activities for the six months and initiate various public policy proposals. At the level of the Council of Ministers, the foreign minister (or another portfolio, depending upon the specific agenda) of the member state holding the rotating presidency plays a leading role.

Emil J. Kirchner, Director of the Center for European Studies at the University of Essex and a long-time observer of European Community/Union politics, has written a timely and important evaluation of the decision-making process at the level of these Council Presidencies and the offices' role in, and impact on, the European integration process, the management of international cooperation, and institutional community building.

The author, through a judicious use of archival material and personal interviewing, along with an analysis of decision-making theory and the various theoretical approaches to political integration, examines how the Council Presidencies impinge on the integration process, especially since the Single European Act and the two 1991 Intergovernmental Conferences (on the European Monetary Union and European Political Union). Assessing the balance and distribution of power between the actors at the Union and national levels, the author consistently refers back to the basic question, Is the Council Presidency primarily a device to maintain and strengthen "national control" over decision making within the European Union, or do the offices go beyond national prerogatives and enhance integration?

The negotiations that led to the Single European Act and the two Intergovernmental Conferences are interpreted by the author as requiring a new approach to, or understanding of, the relationship between the Union-level institutions and the member states. The author concludes that European Union decision making is not solely a transfer of powers from the national capitals to Brussels but also a sharing of competencies between national and Union institutions and a "pooling of sovereignties" among the member states that evokes, at least for me, the American phenomenon of "cooperative federalism."

Kirchner has succeeded in presenting a most readable assessment of the dynamics of European integration, but this success has, in a sense, limited the book's overall attractiveness. *Decision Making in the European Community* is highly useful and informative to the professional well versed in the intricacies of the European Union and to the advanced graduate student, but it is not recommended for the general student, the public, or as a text.

Cleveland State University LEON HURWITZ

Party Policy and Government Coalitions. Edited by M. J. Laver and Ian Budge. New York: St. Martin's Press, 1992. 448p. $59.95.

Legislatures in the Policy Process: The Dilemmas of Economic Policy. Edited by David Olson and Michael Mezey. New York: Cambridge University Press, 1991. 221p. $39.50.

The two volumes reviewed here report findings and conclusions from ambitious research projects investigating aspects of policy making in a cross-national setting. In both, the research emanates from on-going collaboration by international teams of scholars, the participants individually expert in the politics of particular countries and guided, collectively, by cross-national research strategies. The editors lay out first the theoretical and methodological approach of the research, followed by applications in various national settings. They conclude with a chapter summarizing results across cases. The projects are differentiated both by institutional focus and the role assigned to policy in the design of the research. The Laver and Budge collection seeks to determine whether "policy matters" in the decisions made by parties to participate in the formation of governing coalitions. In contrast, the Olson and Mezey anthology explores ways in which legislatures may independently influence the making of economic policy in institutional contexts where political executives have become preeminent. The commitment to cross-national comparison suggests that the goal of both projects is to establish theoretical propositions by empirical generalization.

The projects depart from quite different theoretical and methodological traditions. The Laver and Budge work is rooted in three decades of formal and empirical work investigating coalition processes and behavior. While a certain mathematical elegance attaches to the coalition theories that have been developed, attempts to verify propositions empirically have generally produced disappointing results. Laver and Budge argue that this weakness stems from a general failure to take sufficient account of contextual constraints in adapting theoretical concepts for empirical testing. Thus they develop two expectations regarding coalition behavior in cabinet-forming contexts: that the motivation of actors (parties, generally) is "policy seeking" rather than "office seeking," and the governments that form will be "policy viable" (not constrained by numerical criteria). Drawing upon cross-national longitudinal data coded from party manifestoes and coalition policy declarations, a number of hypotheses regarding these expectations are tested.

Lacking the tradition of formal theoretical work that supports the coalition policy research, the Olson and Mezey project relies upon the largely descriptive literature on comparative legislative behavior to generate a series of hypotheses designed to investigate the ways in which legislatures matter in the making of domestic economic policy. With policy activity (sometimes, policy participation) defined as the dependent variable, 16 hypotheses are stated. The hypotheses are grouped into three general categories to focus on particular contextual variables believed to condition legislative policy activity. These include extra-parliamentary institutions and actors, the internal structure of legislatures, and particular attributes of economic policies. The authors of individual chapters discuss a variety of specific policies and policy processes within the general framework established by this set of hypotheses.

With respect to the establishment of cross-national empirical generalizations in their areas of investigation, both works are, overall, disappointing. This is especially the case with the Olson and Mezey study. Their research design is explicitly and self-consciously structured as a linear regression problem where the objective is the cross-national explanation of variance in legislative policy activity/participation. The hypotheses they advance state a series of bivariate relationships suggesting that their testing will indicate an appropriate multivariate equation explaining legislative policy activity. However, no set of cross-systemic (equivalent) indicators is developed to measure the variables stated in the hypotheses that guide research nor is there a multivariate equation that emerges from the within-country assessments of the bivariate relationships. As such, the descriptions and analyses presented in the individual chapters of this work are not generalizable across cases.

The Laver and Budge effort, in general, avoids this problem. The research design calls for the creation of policy spaces from data sets that use common categories for coding party election manifestoes and government policy declarations, over time, in each of the countries considered. These data are reduced by factor analytic techniques to produce for each of the countries studied a series of policy maps locating the major parties in both one-dimensional and twenty-dimensional policy space. The policy "distance" separating the parties then provides the basis for testing a number of hypotheses relating to coalition formation and policy payoffs. While analysis is confined to individual systems (countries), the design of the research allows results to be directly comparable across countries and across time.

The problem with the Laver and Budge project lies in interpreting the results that are produced; they are either equivocal or disconfirmatory of initial expectations. Indeed, policy-based predictions of coalition formations are poor overall and the policy preferences of governing parties are not reliably reproduced in government policy declarations nor does membership in a cabinet guarantee that government policy will reflect party preferences better than being in opposition. More to the point, Laver and Budge find no pronounced cross-national patterns from the country analyses regarding the specific expectations stated in the research design. Thus, it is not that the results are not generalizable, it is rather that they do not generalize. Since it is scarcely conceivable that policy "doesn't matter" to parliamentary actors when they bargain over membership in government and making policy, the lack of consistent results is discouraging. One suspects, as do the editors themselves, that the main difficulty lies in the analytic and methodological choices that were imposed in executing the research design.

The two projects reviewed here point to a central dilemma of comparison, namely, eliminating sources of residual variation in hypothesized relationships. In a classical formulation more than two decades ago, Przeworski and Teune noted the threat to scientific generalization that is inherent in reliance upon idiosyncratic "country factors" to explain cross-national political phenomena, and they exhorted us, where possible, to substitute variables for country names. Thus, to say that an hypothesized relationship holds *except* in countries X and Y, where some other result is observed, does not invite us to explore the nature of deviance in such cases so much as to search for common (universal) factors that account for cross-national variation. This methodological commitment defines the role of "country experts" in cross-national research projects: namely, to aid in establishing the equivalence of indicators that are selected to measure theoretical concepts. The object is to moot cultural and institutional particularities by controlling their effect, allowing empirical generalizations to be established.

Without a guiding theory of legislative policy influence, the Olson and Mezey project relies upon its contributors to give country-specific interpretations of the policy process. While the research is loosely organized around a series of hypothesized relationships, no elaboration of basic concepts to establish common categories for analysis seems to have been undertaken, nor is there a consistent measurement strategy applied to the various country analyses. In short, no attempt is made to establish cross-national equivalence for either basic concepts or their indicators. The theoretical and methodological weaknesses of this project reduce the role of the country experts to that of providing their own descriptions of national practices, and herein lies the value of the work. To a greater or lesser degree, the various chapters of this volume provide contextual detail and a qualitative understanding of the institutional role of legislatures in the policy process, helping to build a descriptive base for the eventual development of empirical theory.

For Laver and Budge, the guidance provided by traditional coalition theory is at the outset judged deficient for neglecting the influence of contextual constraints operating in national settings. Thus for example, the central theoretical concept of "winning coalition" is replaced in this research by the contextually determined notion of a "policy viable" coalition. This reformulation, produced jointly by the editors and the team of country experts, seeks to nullify the effect of idiosyncratic system factors by creating a theoretical concept that is equivalent cross-nationally, as Przeworski and Teune would have advised. In spite of such efforts, however, the importance of the Laver and Budge project is vitiated by the failure of cross-national patterns to emerge from empirical analysis. The consequence is that the explanations of policy relevance for behavior they offer is significantly dependent upon the judgment of their country experts regarding the influence of system specific factors.

The projects considered here begin, respectively, with the belief that "policy matters" or "legislatures matter." It would, however, appear more to the point to say that "country matters." The value of both works, then, is found in gaining an appreciation of the ways in which similar phenomena are manifested differently across countries. Both works document the importance of contextual constraints on political processes and behavior, and (in the Laver and Budge project particularly) point to the difficulties they pose for the establishment of cross-national empirical generalizations. If, as was observed earlier in this review, we can evaluate the results from these projects to have been disappointing, it is as pertinent to observe that cross-national research is not simple. These and other projects that involve international scholarly cooperation continue to be urgently needed in the effort to generalize knowledge of important political processes and behavior.

University of Wisconsin, Milwaukee ERIC C. BROWNE

Germany Divided: From the Wall to Reunification. By A. James McAdams. Princeton: Princeton University Press, 1993. 250p. $29.95.

German Unification in the European Context. By Peter H. Merkl. University Park: Pennsylvania State University Press, 1993. 448p. $65.00 cloth, $18.95 paper.

Perfect hindsight has led many scholars and practitioners alike to conclude that Germany's division was always destined to be temporary. Politicians in Bonn wrangle over the proper share of credit for having pursued the strategy that made reunification happen. A. James McAdams reminds them all just how stable the country's division into two separate states appeared for several decades. He gently chides many for conveniently forgetting that much scholarship, as well as an entire generation of diplomatic activity, rested on the premise that that division would last. His book's central aim is to explain how partition came to be accepted even (or especially) by Germans themselves.

McAdams suggests that domestic political processes in Bonn and East Berlin, however different in character, both contributed significantly to this gradual process of adaptation to the status quo. McAdams defines process broadly to include the interaction of a variety of domestic political factors, including the interests and goals of top government and party leaders.

Germany Divided advances decade by decade through to the 1990s, though the era that McAdams treats in greatest depth is the 1980s. In each section, he pays detailed attention to the pronouncements and actions of political elites in both German capitals. The author shows how Bonn and East Berlin each gradually abandoned the belief that national division could be ended on terms most satisfactory to its own vital interests.

McAdams's work rests on a wealth of research and is thoroughly documented. One of the book's most important sources is a set of interviews that the author conducted with top figures from the former East German establishment as part of an oral history project on that regime deposited at Stanford University's Hoover Institution.

Germany Divided makes another unique contribution to scholarship on the preunification period by demonstrating quite convincingly just *how much* of what led to that event was inadvertent, even a result of miscalculation. The East's often overconfident and panicky yet always myopic communist elite was busily digging its own grave by loosening, tightening, and again loosening emigration or travel restrictions. And West German political parties, understandably anxious to preserve a sense of stability and normalcy (which was also popular with voters), placed a premium on the "doable" things: while describing the division itself as deleterious, they put ideas of unification on the back burner in favor of practical cooperation with the regime in East Berlin. Like a budget deficit, national division had many sincere critics, but almost all found a way of living with it.

Many scholars of former East Germany have long held to the view that the Communist regime generated some legitimacy, making it less than totally dependent on Soviet military support. Their critics have contended that this premise blurred the fundamental distinctions between a police state and a liberal democracy, thus obscuring the major reason for the East's total collapse. *Germany Divided* does devote equal attention to domestic political processes in the two states but also shows how those features which set them apart also affected relations. Nor does its thesis necessarily view domestic politics as the sole determinant of each country's policy toward the other in the years before 1989. Whatever view one takes of the highest-charged scholarly debate to emerge from unification, McAdams's careful, thorough analysis contributes in a significant way to a better understanding of the dynamics that moved German–German relations for decades.

Where McAdams focuses on the impact of one set of factors in the shaping of policy during past decades, Peter Merkl's study, *German Unification in the European Context*, offers a broad examination. While that might make for a somewhat less compact thesis, the book more than compensates with a breadth of scope encompassing past and future—as well as domestic and international—dimensions of the German Question. Moreover, Merkl frequently returns to a basic, underlying theme, namely, his view that modern Germany's stake in a prosperous, stable environment should reassure those given to concern about unification and its impact on Europe. He repeatedly stresses, for example, that any signs of nationalistic euphoria during the unity drama were restricted almost exclusively to the older generation, while even "flag-waving patriotism" of the American variety has remained rare among younger Germans.

Unlike Elizabeth Pond's *Beyond the Wall* (1993), Merkl's book is not an analytical narrative account of events in 1989–90 but a detailed interpretation of unification. It is structured thematically, though the treatment of each separate topic moves in a generally chronological fashion (at the cost of some unavoidable repetition). In this manner he covers the general erosion of the Soviet bloc, the rise and decline of communist East Germany, the West German response to the historic opportunity of 1989–90, the constitutional order of united Germany, the economic problems associated with unification, the reaction of other countries to this sweeping change in the structure of Europe, and finally the role this "new" country will play in the continent's (d)evolving security regime. Toward the end, the author credits Chancellor Helmut Kohl "for steering the country through the shoals of reunification" (p. 398), contending that he had no choice but to proceed hastily lest international and domestic resistance (including the reluctance of taxpayers to foot the bill) mounted.

In over four hundred pages, few issues are left untouched; the author provides a wealth of documentation, including, above all, survey data on almost every question treated. At the same time, he does not hesitate to enrich his analysis with personal reflections and an occasional poetic turn of phrase, such as the suggestion that a few guard towers on the old German–German border be preserved "as a monument to the living and a reminder that one person's myth can be another person's dungeon" (p. 421).

To be sure, perhaps not all readers will be fully reassured about the future of German politics especially in light of a recent upsurge in right-wing radicalism. Others might remain unpersuaded by the slightly overdrawn contrast between Anglo–American jingoism and idealistic German pacifism. Yet this book makes a major contribution to understanding the multidimensional character of the German unification process and its various, lasting implications. In particular, the author makes a convincing case that (appearances to the contrary) unity did not merely represent an extension of West German institutions and practices to the East: in almost all respects, much remains that divides these two halves of the country, including (as Merkl again documents with survey data) a mutual antagonism in popular attitudes across the old border. The author leaves a clear impression that ending Germany's partition was the easy part; overcoming the effects of four decades spent in entirely different political and social systems is a task that will take much more time to complete, with complete success by no means assured.

Aside from the general topic of Germany unity and quality of scholarship, these two books share certain other similarities. To be sure, neither author is primarily concerned with the German issue's relevance for broader theoretical issues in comparative politics or international relations. Yet both emphasize the degree to which politicians (above all, in West Germany) internalized the very concerns about their country's impact on European stability that long made neighboring states worry about unification. That phenomenon may ultimately be the one legacy of Germany division that ensures the success of German unification.

College of William and Mary CLAY CLEMENS

The Sacred Cause: Civil-Military Conflict over Soviet National Security, 1917–1992. By Thomas M. Nichols. Ithaca: Cornell University Press, 1993. 259p. $32.50.

Like many other features of the Soviet political order, Soviet civil-military relations have recently attracted new scholarly scrutiny. Even during the comparatively stable Khrushchev and Brezhnev eras, Western analysts' disagreements about the nature of Soviet civil-military relations had important practical implications. But the participation of senior Soviet officers in the abortive August 1991 coup and the role of military units in the October 1993 showdown between President Yeltsin and the Russian parliament have given these long-standing debates greater currency.

In *The Sacred Cause* Nichols makes a significant contribution to a better understanding of recent trends in Soviet and Russian civil-military relations. Although the book covers the entire span of Soviet history, most of it deals with the period from Khrushchev through Gorbachev. Drawing on a wide range of Russian-language sources, Nichols concentrates on the development of military strategy, weaving into the account a substantial amount of information about other topics. He rightly pays close attention to the civil-military contest over the proper relationship between "military science," a domain in which the military was acknowledged to have special expertise, and "military doctrine," in which the Party traditionally claimed the final say.

The book's main thesis is that civilian Party officials and Soviet military officers were divided by conflicts inherent in the Communist system. Due to the absence of any meaningful Soviet constitutional tradition and to the Party's formal advocacy of transcendent political principles, civilian leaders could never establish unconditional control over the military. In the terminology of Samuel Huntington's *The Soldier and the State* (1957), Party leaders resorted to "subjective" rather than "objective" means of control. That is, they relied on such devices as police surveillance, political criteria of promotion, ideological indoctrination, and occasionally promulgation of military policy by Party fiat, rather than on an political-military division of labor that granted officers full professional autonomy in exchange for a military commitment not to intervene in politics. This approach, however, had unintended consequences. In the twilight of Soviet power, an officer corps steeped in Marxism-Leninism was pitted against an increasingly pragmatic Party elite. The Army became "the watchdog of the Party rather than vice versa," producing a lasting confusion of military loyalties that "may be among the greatest threats to the emerging post-Soviet democracies" (pp. ix–x).

This interpretation is novel and thought-provoking. It is a useful attempt to evaluate Soviet civil-military relations by comparative standards of constitutional loyalty not generally applied to the USSR by Western analysts. Prof. Nichols underscores the mingling of Soviet Party and military officials in decisionmaking bodies such as the Party Central Committee—and, one might add, in the Party Politburo after 1973. The author could have further buttressed his argument for the Party's reliance on subjective mechanisms of control by giving fuller attention to the use of the secret police to prevent potential challenges from the military. The blurring of the institutional boundaries between the civilian and military realms has become especially pronounced in the post-Soviet period. Due partly to a faulty understanding of the concept of the separation of powers (at least one-sixth of the deputies in the lower house of the new Russian parliament are employees from executive government agencies) military officers have been elected to legislative bodies and have plunged into bitter debates about Russia's socioeconomic crisis.

Although Nichols is right to note the Soviet precedent for this post-Soviet blurring of civil-military boundaries, the depth of the military's actual involvement in civilian matters before the late Gorbachev years remains debatable. In my opinion, the author's general depiction of civil-military relations does not take adequate account of the substantial variations that occurred during the Soviet era, even though his historical exposition notes these variations. For example, he states that the military had a predominant role in the formulation of military doctrine in the 10 years after 1925 and in the 18 years of Brezhnev's rule, which together amount to more than one-third of the history of the USSR. In addition, the book's empirical focus on military policy—the domain in which uniformed officers had the most legitimate grounds to question the judgment of party leaders—makes it hard to assess the degree to which military men actually staked out positions on civilian matters. The most senior commanders undoubtedly had opportunities to "log-roll" on civilian issues during Central Committee and Politburo meetings, and in the early Stalin era a few senior commanders registered objections over the disruptive manner in which agricultural collectivization was implemented. For most of the Soviet period, however, the public pronouncements of senior officers focused almost exclusively on military issues and gave few indications of efforts to influence decisions on nonmilitary matters.

Although this pattern changed in the late Gorbachev years, it is questionable whether national security policy, undeniably the source of sharp disagreements, became the most important bone of contention between Party leaders and military men. To judge by the timing, preserving the Soviet state against the risk of internal collapse was the issue that propelled many senior officers into outright defiance of Gorbachev in 1991. Differences over external threats probably played a secondary role in precipitating military involvement in the attempted coup. In addition, the concept of an ideologically indoctrinated military establishment does not capture the splits which had riven the officer corps by 1991 and which Nichols himself notes in passing. If the 1990 voting behavior of various military delegates in the Russian legislature is any guide, many senior commanders remained loyal to Marxism-Leninism, but many middle-rank and especially junior officers were hostile to Marxism-Leninism (as were a surprising number of low-level Party officials). Adherence to Marxism-Leninism among senior commanders helped trigger the 1991 coup attempt, but disbelief in Marxism-Leninism among other members of the officer corps helped defeat it.

Whether the reader accepts all the propositions contained in the *Sacred Cause*, the book makes a substantial contribution to reassessing the dynamics of the Soviet system and the implication of those dynamics for current upheaval in Russia. Scholars interested in such issues will read it with profit.

Johns Hopkins University BRUCE PARROTT

Citizens, Political Communication, and Interest Groups: Environmental Organizations in Canada and the United States. By John C. Pierce, Mary Ann E. Steger, Brent S. Steel, and Nicholas P. Lovrich. Westport: Praeger, 1992. 256p. $47.95.

This work is self-consciously directed at addressing in an empirical way an important normative issue in democratic theory: How are citizens to face the mounting informational challenges posed by policy issues in "postindustrial" society? Specifically, they ask, Do interest groups help citizens meet the challenges of scientific and technical complexity effectively? The authors claim that they do. Unfortunately, their data fall very far short of establishing that claim. The authors show rather convincingly that environmental interest groups in Canada and the United States are important *information sources* for both policy elites and interest group members. But they have failed to build into their study any measure of effective communication and so cannot equate desire for information or information per se with adequate information or a reduction of complexity. The reason for this gap is that the authors' democratic instincts are too deeply rooted in an economistic model of citizenship. Hence, more (information) is better: the citizen-as-information-consumer demands information; interest groups supply it; ergo environmental groups are good.

The study's fundamental thesis is that "environmental interest groups may act as a unique mechanism for assisting democratic publics in dealing with their need for help in evaluating and employing scientific and technical policy-relevant information" (p. 106). A secondary thesis—which unfortunately occasionally threatens to overwhelm this focus—is that political culture influences the functioning of political institutions (in this case, interest groups). The latter thesis is born out by the analysis in a rather straightforward way; not so the work's primary thesis.

The authors' central thesis, rephrased toward the end of the work, is that "interest groups in both the United States and Canada have the capacity to assist citizens in coping with the problem of the technical information quandary" (p. 186). This thesis is supported by three separate findings: (1) at the macro level, the authors discover some convergence in political culture, which they believe suggests "a common potential for having interest groups play an important political communication role in linking contemporary citizens to the public policy process"; (2) at the micro level, individuals differentiated between different groups as sources of political information and identified *information* as the primary motive for membership in environmental interest groups; (3) at the level of group processes, environmental interest groups were found to "rely extensively in carrying out their activities on the communication of policy-relevant information to their members and lay citizens" (pp. 176–77, 186). The cross-national survey data, supplemented by elite interviews with environmental activists and policymakers, appear to support these three propositions. The problem is that these propositions do not themselves add up to the book's basic thesis.

If the authors wish to equate the citizen's need for information with "a preference for information"—as they often appear to do (pp. 93, 97, 118, 150)—then one cannot speak of interest groups' doing anything more than gratifying an information itch. Talk of "effectiveness," "dealing with complexity" or "coping with complexity" all becomes synonomous with the instrumental delivery of utility-as-information and interest groups are simply (information) satisfying or not, nothing more. Fortunately, the authors do not really believe in this notion of a free market of ideas. In several places, they suggest that there is more to political communication than informational hedonism. For example, toward the end of the book, they note that interest groups may assist individuals learn by "cue-giving, resource-pooling, and influence-exerting activities" (p. 186). The problem is that these activities are not scrutinized here, much less tied to any explicit theory of learning. Nor, it might be added, are these tied to any explicitly *democratic* theory, either. (Does effective communication require any communication *from* the citizen *to* the interest group? What kind?) One finishes this book impressed by the empirical research and buoyed by the findings but wondering just what they mean. "Interest groups can get the information job done," the authors argue. But just what is information? And what is getting the job done?

Although the authors' theoretical grasp exceeds their empirical reach and although their argument is weakened in several places by a lack of conceptual clarity, there is much to commend in this volume: if it attempts to say more than it should, what it does say is important and stimulating. It matters a great deal that citizens' relationship to interest groups appears to be motivated, above all else, by a desire to seek information. If the authors stray beyond the data, they also take us into interesting and even provocative terrain. And if they fail to settle the important issue they take up, they at least take us some of the way there and point to the next step. Interest groups process a lot of information: But how well do they learn? How well do they teach?

Texas A & M University Adolf G. Gundersen

Red Sunset: The Failure of Soviet Politics. By Philip G. Roeder. Princeton: Princeton University Press, 1993. 317p. $49.50 cloth, $16.95 paper.

Accounts of the political system of the USSR have alternated between emphasizing its routinized, bureaucratic character and considering it anarchic, potentially unstable, and prone to personal dictatorship. In the late 1970s, Western analysts became increasingly aware of the substantial decline in the regime's performance, because the enfeebled party leadership was increasingly hostage to the great bureaucracies that the regime had created.

In *Red Sunset,* Philip Roeder renews this line of argument. He finds that the regime grew stagnant because of the strength of the political ties connecting the top party leadership with the powerful bureaucratic interests represented in the Central Committee of the Communist party. He argues that past accounts have paid too little attention to the party General Secretary's dependence upon a base of support in the party machine, the military, the state bureaucracy, and other "second-tier" interests. He advances a concept of "reciprocal accountability" that, he believes, was implicit in the original requirement of the Bolshevik Revolution to delegate power to perform specific functions to bureaucratic and regional power centers. The Stalin period represents an aberration from this "constitution," according to Roeder; but immediately after the dictator's death, Stalin's successors restored the older collective leadership model. Ultimately, however, that very model became an obstacle to deep reform, because the rulers could not introduce significant redistributive change without jeopardizing their own political coalition. As a result, the system lost its capacity to maintain itself. Gorbachev's radical reform efforts simply proved the intrinsic incapacity of the system to preserve itself through democratization: a successful balancer at the outset, Gorbachev finally provoked his allies both on the Right and the Left to abandon him in 1990–91.

Roeder's account has a number of strengths. It provides a theoretically reasoned interpretation of the development of the Soviet political system, which accommodates both the apparent stability of the Brezhnev period and the rapid dissolution of the union in 1991. Moreover, he offers three case studies to demonstrate how Soviet coalition politics affected, and was affected by, policy decisions. The book also has the ambitious objective of proving the utility of a neoinstitutionalist perspective for the study of authoritarian regimes, where their application has been inhibited by the fuzzy, tacit, and arbitrary character of rules and procedures governing political life. In a sense, the book stands the usual problem of social choice models on its head: instead of asking how majority rule can yield stable policy outcomes, Roeder asks how rules under authoritarianism can yield stable political coalitions. In turn, this leads him to reach the conclusion that the very stability of an authoritarian regime, based on a compact among oligarchs, may be untenable if the regime itself cannot cope with serious performance problems.

Roeder wants his model to turn our attention back to purely political processes and away from the more recent preoccupation with state–society relations. In this sense, he returns us to the classical concerns of Soviet political studies: how power and authority are built and maintained, how political coalitions are linked to policy lines, what different patterns of representation on leading party bodies tell us about the balance of power within the party state. On these matters, Roeder builds on, and formalizes, many of the findings of earlier Sovietological literature. But these gains come at a price. Left out of sight are social changes that altered the balance of power between regime and social groups, between center and regions, and certainly between the predominantly Russian political elite and the increasingly self-aware political and social elites in national republics and other ethnic–national territories. The problem of corruption, with its implications for the nationality dimension of politics, is not discussed. Perhaps even more central is the question of property rights. Could any communist party state have permanently reversed the decline of economic performance without establishing a secure property rights regime?

Roeder may, indeed, exaggerate the degree to which the regime operated under accepted rules of the game over the seven decades of its existence. Different leadership phases developed distinctive sets of understandings about legitimate means of seeking power, making policy, removing opponents, rewarding allies. These rules and informal constraints differed less as between the Khrushchev and Brezhnev periods than as between those two periods together and Stalin's reign. The "constitution" of the Stalin period, in turn, only gradually emerged out of the struggles of the NEP and war communism periods. Some scholars have gone so far as to say that there was, in fact, no single "Soviet system" because of the deep discontinuities between the operational codes of various leadership periods. This is surely overstated. But it is likewise misleading to speak of a fundamental "constitution of Bolshevism" unless it is also understood to mean the original Bolshevik project of building socialism in Russia.

Emory University Thomas F. Remington

The Birth of Judicial Politics in France: The Constitutional Council in Comparative Perspective. By Alec Stone. New York: Oxford University Press, 1992. 312p. $45.00.

Judicial Politics in France is the first full-length study in English of France's Constitutional Council. An exhaustive account of a neglected institution, it adds an important chapter to our understanding of French politics. In a careful study of its activities from 1958 to the present, and relying almost entirely on official French sources, Alec Stone traces the Council's gradual transformation from a relatively inactive quasi-judicial agency into a major policy-making institution of the Fifth Republic. Designed originally to assist the president in checking parliament, the Council has evolved into a weapon that legislative minorities have used to check the president and parliament as a whole.

The critical year in this transformation was 1974, when any 60 legislators in the Senate or National Assembly won the right to petition the Council for a ruling on the constitutionality of bills passed by parliament but not yet promulgated as law. With this expanded right of petition (it had previously been limited to the president, prime minister, and presidents of the two parliamentary chambers) and the Council's increasing readiness to annul or alter bills referred to it, parliamentary parties opposed to the government have been able to wrest changes in nonpromulgated legislation from both government and parliament. Stone documents this process in detailed case histories of legislation dealing with the nationalization of industry (chapter 6) and media broadcast policy (chapter 7). From these studies and related data, he concludes that the council's activities have significantly altered French executive–legislative relations, as well as subverted France's historic commitment to the principle of parliamentary supremacy.

In Stone's account, parliamentary minorities refer an exceptionally large number of adopted bills—one out of seven in the 1980s—to the Council for scrutiny on constitutional grounds. These minorities have virtually converted the Council into a third legislative chamber. In nullifying or laying down conditions for the final passage of valid legislation and thus delaying its promulgation as law, they often force legislators to renegotiate the terms of earlier agreements. Even threats to take bills to the council prompt parliamentary majorities to accept legislative changes they would otherwise reject. Stone invents fancy terms such as *autolimitation* and *corrective revision* to describe parliament's reaction to the council's review of legislation, a response that transposes debates on policy into constitutional arguments and legalistic quibbling. The consequence of this process is the "judicialization" of the legislative politics and the "politicalization" of constitutional review.

As its subtitle suggests, *Judicial Politics in France* introduces a transnational perspective. In part 3, "Toward a Comparative Understanding of Judicial Review," Stone compares the Council with the German, Austrian, and Spanish constitutional courts. Part 3, however, is little more than an addendum to an otherwise exemplary study of a single institution in one country. Its burden is to show that constitutional courts empowered to nullify legislation on abstract judicial review (i.e., review of legislation outside the framework of ordinary litigation) are engaged in an activity more legislative than judicial. Other constitutional courts, he finds, are engaged in a genre of political decision making no different from the activities of the council. In the end, Stone views judicial review as an undertaking rooted entirely in politics, and that view, he thinks, should drive the comparative study of judicial review.

Although provocative and interesting, part 3 will be seen as less than convincing by some constitutional scholars. Stone recognizes that constitutional review in France differs from its practice in other countries—i.e., that constitutional review of unpromulgated legislation is different from the judicial review of laws already in force—but he nonetheless presses the work of the latter into the mould of the former. A glance at the notes may explain the reductionism. When Stone writes about the Council, he exhibits a surefootedness grounded in primary sources; but when he shifts attention to other countries, he betrays a reliance on secondary materials of limited value. What else would explain his acceptance of the view that German constitutional decision making is driven as much by partisan politics as the work of the Constitutional Council? What else would lead him to the conclusion that abstract judicial review is all politics and no law?

In fairness, however, Stone's comparative report should be seen as less a definitive account of the politics of judicial review than an ambitious effort to define a framework for the transnational study of judicial review. Stone is engaged in a worthy attempt to join constitutional law to the study of comparative politics. Constitutional scholars should applaud this effort and the strategy generally of deploying public law for the purpose of providing a more complete and nuanced account of policy-making in constitutional democracies. By the same token, as he continues his work on constitutional courts outside of France, Stone may begin to qualify his account of these tribunals and perhaps even concede that constitutional interpretation, at least in some legal settings, has its own integrity above and beyond politics.

As a comparative politics scholar, Stone is a *rara avis*. He is one of the first in the subdiscipline of comparative politics to take courts seriously, and he appropriately credits Martin Shapiro's work on "political jurisprudence" for inspiring him to turn his attention to judicial institutions. This is a happy development. Up to now, the comparative study of law and courts has been the preserve of public law scholars within political science, just as they and other legal scholars have dominated the study of foreign constitutional courts. The heavy doctrinal and normative emphasis of these legal scholars may be one reason comparative politics has failed to give their work the attention it deserves.

Stone, by contrast, views constitutional courts and their decisions as normal parts of the political process that any inclusive account of politics must consider. He shows that these courts are important in defining the rules of politics and that an account of their work and influence are essential to a fuller understanding of the political systems in which they operate. On the other hand, these courts also define the goals and aspirations of the constitutional polities in which they operate, a function that requires some distance from ordinary politics and some measure of devotion to law's integrity and the principle of judicial independence. In this respect, finally, Stone's account of the Constitutional Council's political jurisprudence might be contrasted to John Bell's recent study of its normative jurisprudence (*French Constitutional Law*, 1992).

Judicial Politics in France prompts a concluding comment on style. Stone has a tendency to italicize every important point in the book. Almost every page features italicized words or remarks, a practice that draws attention to itself and annoys the reader. Ordinary English words should not be italicized. The author, if he has any mercy at all, would allow his readers to do the underscoring.

University of Notre Dame DONALD P. KOMMERS

INTERNATIONAL RELATIONS

Japan in the Posthegemonic World. Edited by Tsuneo Akaha and Frank Langdon. Boulder: Lynn Rienner, 1993. 295p. $40.00 cloth, $17.95 paper.

The Challenge of Japan: Before World War II and After. By Nazli Choucri, Robert C. North and Susumu Yamakage, New York: Routledge, 1992. 383p. $59.95.

The two books under review are most welcome additions to the increasing literature in English on Japan's foreign policy, of which Gerard Curtis's collection, *Japan's Foreign Policy after the Cold War* (1992) is a notable example. They give readers fresh readings of Japan's foreign policy at a time when the world is changing very steadily.

Despite their differences, they share a common preoccupation with hegemony and a common underlying question: Once the U.S. hegemonic era is gone, is Japan likely to be the next hegemon?

Their approaches are quite different. *The Challenge of Japan* focuses on the pre–World War II period and tries to see the post–World War II period with the same internally coherent conceptual framework. *Japan in the Posthegemonic World* focuses on the contrast between the pre–Cold War and post–Cold War periods, with the contributors focusing more or less on bilateral relations of Japan in specific policy areas. *The Challenge of Japan* argues that what the authors call *lateral pressure* (i.e., the demand for outward expansion derived from the configuration of population, resources, and technology) determines Japan's foreign policy, using simultaneous equation models based on macroeconomic time series data. In other words Japan's large expanding population, relatively poor resource endowments combined with a rise of demand for resources due to steady industrialization, and the advances in technological level are major determinants of lateral pressure for Japan. Whether it takes military or economic outward movement is determined by international environments. For the pre-1941 period, lateral pressure mounted, and Japan manifested its lateral pressure through military aggression. For the post-1945 period, the framework does not work very well. The authors admit this by saying, "In the long run, continued success in exporting manufactured goods was important in diverting Japan's lateral pressure away from the military form or mode (p. 294). *Japan in the Posthegemonic World* argues that "there is no hegemon to rescue us, no *deus ex machina*" and that "it is no longer a matter of either U.S. or Japanese hegemony; it is cohegemony (bigemony) or, a little more generally, a Pax consortis'." (pp. 88, 262).

The credibility of the approach and framework of *The Challenge of Japan* would have been further enhanced if the authors had confined the data to the pre-1945 period, while evoking the same conceptual framework both for the pre-1945 period and as they look to Japan's ascendance and its possible explosion in the future. In contrast to this, *Japan in the Posthegemonic World* does not well integrate later, empirical chapters with the first two or three chapters concerning the theory of global hegemony and the nature of Japan's power (the relatively sanguine pursuit of national interests combined with "a more constructive notion of leadership in a world of complex interdependence" [p. 66]). Further efforts could remedy these shortcomings. For *The Challenge of Japan* one could develop the conceptual framework of lateral pressure on the basis of global market access in trade and investment, with population, resources, and technology components kept underneath in an era of protectionism and regionalism on the one hand and global environmentalism on the other. The conceptual framework could be devised more directly to the changed international environments. For *Japan in the Posthegemonic World,* we might justifiably hope for a single-authored volume on the same theme. Although much has been written on Japan's foreign policy, still more remains to be done. Despite the increasing demand for books on the subject, the relative scarcity of specialists in Japan's foreign policy, especially in North America, leaves big lacunae unfilled for some time to come.

University of Tokyo TAKASHI INOGUCHI

The Presidency and the Rhetoric of Foreign Crisis. By Denise M. Bostdorff. Columbia: University of South Carolina Press, 1993. 316p. $34.95.

As George Bush's meteoric public approval ratings in the wake of the Persian Gulf war aptly demonstrate, leadership in foreign crises from the Oval Office can provide clear domestic political benefits. But such approval can also be a short-lived aberration that flashes across the electoral scene like a Scud on a starless night. Denise Bostdorff correctly notes that presidents, although they do not create the world they face, play a large role in shaping public perceptions of that world by promoting, defining, and (sometimes) resolving foreign crises. Presidents influence world events and public perceptions not only with military and diplomatic initiatives but also with sometimes carefully chosen words and symbols. Their words and symbols then influence the policies they choose.

Bostdorff devotes her attention to the public rhetoric of presidents in foreign policy crises, arguing that we can gain insight into both the presidency and the men who have occupied it through close textual analysis of their speeches. The research design is both simple and ambitious: she picks one foreign crisis for each president from Kennedy to Reagan and looks closely at all relevant public statements by the president. She also looks at internal memos promoting particular policies and rhetorical statements and official statements by sanctioned spokespeople. In this regard, Bostdorff's work is a real advance over previous studies of presidential rhetoric focusing on "major" speeches, such as State of the Union addresses or other formal addresses. She recognizes that a few unguarded comments can do as much to influence public perceptions as the most carefully crafted formal address.

To her credit, Bostdorff does not present rhetoric as an independent creation, but attempts to situate each discussion in both the foreign and domestic politics of the time. She begins each chapter by setting the international scene and the domestic pressures that the president faced. It is impossible not to admire the sheer volume of materials (archival documents, public papers, and a range of historical and analytical treatments) that she synthesized in order to assess the real importance of presidential rhetoric. At the same time, the foreign policy specialist or historian may find some of the necessarily brief treatments of context frustrating. For example, Bostdorff notes, in her chapter on Jimmy Carter and the Iranian hostage crisis, the speculation that candidate Ronald Reagan cut a deal with the Iranian government to ensure that Carter's efforts would not succeed while he was in office. Bostdorff provides no critical perspectives or evaluations of these charges.

The heart of Bostdorff's analysis is a formal dissection of presidential rhetoric, using a method developed by Kenneth Burke, a theorist of rhetoric. Putting language at the center of concern and finding motivation *within* the texts, Bostdorff attempts to discover how each president portrayed the "crisis" situation, the style the president employed in managing the crisis, and the bases of identification of allies and enemies he encouraged in the public. Through this method, she intends to illuminate differences in style and substance among the presidents and derive some insights about the nature of crisis promotion in contemporary presidential politics. (Bostdorff's use of Burke's method will be new to many political scientists, who may find it difficult to wade through the textual analyses.)

The differences, as expressed in close readings of each case, are subtly drawn and produce sometimes surprising insights. Bostdorff finds that President Ford, for example, departed from the standard practice of crisis promotion when the *Mayaguez* was seized off the coast of Cambodia. Whereas other presidents inflated such events, promoting crises to promote their own leadership, Ford himself was silent on the matter. His public affairs staff provided only brief descriptions of events as they developed. When the crisis ended, Ford described the events and his actions only briefly. Bostdorff suggests that Ford's silence was a deliberate rhetorical strategy to bolster his standing as a national leader. Unelected, Ford desperately sought to cultivate legitimacy as president by allowing his staff to portray him as competent, active, and busy responding to the problems of the day. Aware of his own difficulties with public speaking, Ford limited his public speeches and even his answers to questions at press conferences. In this way, he was able to maximize the political benefits attendant to the crisis—by saying less.

In stark contrast, Jimmy Carter initially spoke frequently about his handling of the Iranian hostage crisis, emphasizing publicly the principles by which the United States would manage the affair. At the same time, by constantly reiterating principles but rarely reporting progress, he undermined public perception of his efficacy as a leader. "The idealism of Carter's rhetoric," Bostdorff writes, "also constrained his crisis management by narrowing the range of policy options he could pursue to those that he deemed consistent with the moral principles he set forth" (p. 159).

Of course, the analysis of Carter points to a critical question Bostdorff dodges: To what extent is rhetoric a reflection or a cause of presidential policy in a foreign crisis? Bostdorff considers explicit political planning in conjunction with the psychic vulnerabilities of American presidents, without placing anything above the texts of presidential speeches, both improvised and committee-crafted. The real role of rhetoric as reflection and as source of, or substitution for, policy remains to be explored.

John Kennedy, she argues, established formal mechanisms and legal justifications for crisis promotion and management, an infrastructure that remains. While there are recurrent patterns in presidential rhetoric, the infrastructure has grown and strengthened. Bostdorff concludes: "The relationship between power and discourse appears to be symbiotic. The president's control over foreign affairs allows him to indulge in crisis promotion; at the same time, crisis promotion frequently helps the commander-in-chief to expand and consolidate his power. This is a dangerous combination indeed" (p.234). Scholars of the presidency—and of foreign policy more generally—owe Denise Bostdorff a debt for framing this problem of modern democracy so sharply.

University of Michigan David S. Meyer

The State of the European Community, Volume 2: The Maastricht Debates and Beyond. Edited by Alan W. Cafruny and Glenda G. Rosenthal. Boulder: Lynne Rienner, 1993. 435p. $49.95.

The 1992 Project and the Future of Integration in Europe. Edited by Dale L. Smith and James Lee Ray. Armonk: Sharpe, 1993. 260p. $45.00 cloth, $17.50 paper.

Part of the professional creed of political scientists is the belief that narrowly focused academic volumes—preferably monographs—are the most worthy. Yet this has not consistently been true of the scholarly study of the European Community. To be sure, the field was launched in the 1950s and 1960s by a series of monographs by Ernst Haas, Leon Lindberg, Joseph Nye, and others. The next two decades, however, saw a series of substantively broad-ranging yet theoretically sophisticated volumes assessing the state of the European Community, including those edited by Leon Lindberg and Stuart Scheingold, Helen and William Wallace and Carole Webb, Alberta Sbragia, and (most recently) Robert Keohane and Stanley Hoffmann.

Like its distinguished predecessors, each of the books reviewed here has two stated purposes: to document the current condition of European integration and to theorize about it. *The 1992 Project,* edited by Dale Smith and James Lee Ray, contains 12 essays, four on integration theory and eight on specific topics ranging from defense cooperation to the role of subnational development agencies. Without striving for comprehensive coverage, the authors uncover a wealth of interesting information. Several essays cut sharply against the grain of conventional wisdom. John Conybeare rigorously grounds his skepticism about the future of transatlantic trade relations in optimal-tariff theory. Paulette Kurzer offers a provocative analysis of the European Monetary System as instrument and indication of the "passing away . . . of social democracy and the postwar settlement" in small corporatist democracies (p. 140). David Garnham traces in intriguing detail the Anglo–French flirtation in 1990 with an anti-German alliance. Russell Dalton and Richard Eichenberg seek to revitalize the study of public opinion and integration.

As its title suggests, *The Maastricht Debates and Beyond*, edited by Alan Cafruny and Glenda Rosenthal, does aim at comprehensiveness—and splendidly achieves it. Second volume in a biennial series sponsored by the European Community Studies Association, it contains a rich and timely collection of 23 essays on European Community developments over the past two years. The reader senses the unusually astute eyes and firm hands of two editors committed to maintaining a strong standard of selection and execution. The essays are uniformly concise and focused, yet lively.

Cafruny and Rosenthal seek to provide a detailed overview of issues ranging from immigration to telecommunications and (though with less enthusiasm than Smith and Ray) to provide social scientific explanation of those events. In the service of the first goal, special mention must be made of the useful, detailed review of the Uruguay Round negotiations by Finn Laursen; Sir William Nicoll's analysis of the Maastricht negotiations, which reflects an intriguing mixture of federalism and skepticism; and the especially thorough treatment accorded Germany and the EMU negotiations. On the latter topic, David Andrews and Wayne Sandholtz document (as widely reported at the time) the initial success of Helmut Kohl's personal efforts to strengthen, at least on the margin, the German commitment to monetary integration. Particularly insightful is Paul Welfens's analysis of the macroeconomic contradictions of Bundesbank policy.

Here, too, are contributions that employ social-scientific analysis to generate unexpected insights. Dale Smith and Jürgen Wanke make elegant use of descriptive statistics to determine who wins and who loses by "Europe 1992." Walter Goldstein provides a tough-minded review of the European Community's internal tension between market liberalism and industrial policy. Robert Geyer carefully compares British and Norwegian socialism in the new Europe.

Yet, while both books mention fundamental theoretical questions of European Community studies, they ultimately decline to provide distinctive answers to them. The current conjuncture of the Community has raised many such questions, many closely linked with enduring concerns of comparative and international politics: How does the relative power of the Commission and member governments vary across issues? Does Community integration strengthen or weaken the state internally? Under what conditions and in what ways does membership in the Community alter the relative influence of domestic political groups? Has national support for European integration undergone a fundamental process of politicization, transforming a "passive" into an "active" consensus? Under what conditions do European countries share an underlying interest in moving from "negative" to "positive" integration, that is, from market liberalization to the joint provision of public goods like macroeconomic stability, environmental protection, and a common defense? Is the Community evolving into a new sort of polity, or do present trends presage an extension of long-term patterns?

If a theoretical theme can be discerned in these two volumes, it concerns the last question. Both books stress the increasing complexity and diversity of the European Community. For Smith and Ray, complexity emerges, above all, from the Community's institutional structure. The European Community is one of the few international organizations with a supranational officialdom possessing significant, though limited, autonomy. With the inclusion of another actor and forum, what Robert Putnam has termed a "two-level game," played by statesmen on domestic and international "boards," becomes a "three-level game." (Indeed, Smith and Ray hint that Community *external relations* and *transnational coalitions* add a fourth and fifth level!) At the same time, the Commission seeks to empower subnational actors, thereby further diffusing political power. In his analysis of regional policy in *The Maastricht Debates*, Gary Marks asserts that "we are seeing the emergence of a *multilevel governance*, a system of continuous negotiation among nested governments at several territorial tiers—supranational, national, regional, and local" (p. 392). A causal link between greater complexity in policymaking and greater diffusion of political power is surely an important proposition about the development of the European Community (although, it must be added, a more highly debatable one than either volume hints). My own view is that on balance, far from undermining the state domestically, the Community actually *strengthens* it.

Even if we provisionally accept the hypothesis that complexity is diffusing power in some issue areas, simply to celebrate complexity is to make a virtue of necessity. The next step, an essential one for social scientists, moves from observation to explanation. What is the theoretical significance of complexity and diffusion of power? Under what conditions does the relation hold? What are the implications for integration? Here both volumes leave the field to others. Despite their admirable ambition in this regard, Smith and Ray do not move beyond the metaphor of "three-level games" to theory—retreating, instead, in their concluding chapter, to the reassertion that political union is a sort of European destiny. Perhaps constrained by the format of their book, Cafruny and Rosenthal, too, make little attempt to postulate an overarching model or a common set of hypotheses, contenting themselves, instead, with the modest conclusion that intergovernmental negotiations between member states appears to remain central. Marks asserts only that the starting point must be the assumption that the Community is a "complex, open-textured and fluid" polity. But if this is the theoretical starting point, where is the endpoint?

The problem is not a new one. It closely resembles the predicament facing theorists of "complex interdependence" in the 1970s, who found that the assumption that everything is connected to everything else is, ultimately, no assumption at all. Only the more detailed analyses of trade and capital interdependence since the mid-1980s, firmly grounded in explicit microfoundations, have provided strong testable propositions. The study of interdependence has grown richer as a result. Similarly in European Community studies, analyses such as those found in these two books perform an indispensable function by opening our minds to a wide range of possibilities; but it now falls to theoretically structured empirical analyses to help separate the wheat from the chaff. It is here—more than in reviews of recent events—that social scientists can claim a distinctive comparative advantage over the myriad annual reviews of the European Community that have sprung up in recent years.

There are two complementary paths forward. The first, as Leon Lindberg notes in the Smith and Ray volume, is to develop more explicit, generalizable theories of economic interest, interstate bargaining, and international regimes. Modern theories based on specified causal mechanisms at the micro level offer many of the tools to move beyond ideal-typical description to explanation. Recent works by Giandomenico Majone and George Tsebelis, for example, suggest subtle, counterintuitive consequences of complexity in the Community. The second path is to pursue serious cross-issue, cross-national, and cross-temporal comparative analysis. The sudden success of the 1992 initiative has encouraged, among scholars, a historical myopia in European Community studies that is more pervasive, yet considerably less justified, than that of the 1960s. Moreover, there is a dangerous tendency to generalize from specific issues or single decisions. Only explicitly comparative analysis—within the Community and between the Community and other organizations) can tell us whether the current conjuncture is the continuation of old politics under new circumstances or the emergence of a new institutional patterns of politics altogether.

This sort of sustained social-scientific inquiry is quite consistent with the careful analysis of recent events found in these two books. One hopes that forthcoming volumes of this type will serve as vehicles to unite the two.

Harvard University Andrew Moravcsik

The New Global Economy in the Information Age: Reflections on Our Changing World. By Martin Carnoy, Manuel Castells, Stephen S. Cohen, and Fernando Henrique Cardoso. University Park: Pennsylvania State University Press, 1993. $25.00 cloth, $12.95 paper.

For the reader looking for new arguments or evidence about change in the world economy, this book will be a disappointment. On the other hand, it offers a succinct, albeit one-sided, summary of familiar arguments and data about the world economy in four areas (each treated by a different author): the new world informational economy, the continuing power of multinational enterprises, the decline of American hegemony, and the marginalization of the poor developing countries.

The underlying theme is that national policies still matter in the new world economy. Manuel Castells, in the opening essay, argues that the "'developmental state', a code word for Japan, played a fundamental role in affecting the transformation of the world economic structure in recent years" (p. 23). Martin Carnoy writes that although "multinational enterprises are powerful economic actors, . . . nation-states can also be key actors in shaping what multinationals do," especially in promoting local human-capital development and research-and-development financing (p. 93). Stephen C. Cohen is even more emphatic that national policies dictate success. He points primarily to a "set of institutional innovations" pioneered by Japan that employ the state as gatekeeper, the keiretsu industrial organization, and the targeting of key technologies and industries (p. 100). Finally, Fernando Henrique Cardoso concludes that developing nations, too, can succeed if they pursue the right policies: "Either the South (or a portion of it) enters the democratic-technological-scientific race, invests heavily in R&D, and 'endures the information economy' metamorphosis, or it becomes unimportant, unexploited and unexploitable" (p. 156).

The theme of the book is directed against those who claim that freedom (individualism) and global markets have triumphed and that national policy choices must be subordinated to economics. This theme is welcomed as a corrective to an earlier overemphasis on interdependence and dependence, to which some of these same writers contributed, particularly Cardoso. But the authors are a bit one-sided in what they consider the right national policy choices to be.

They see Japan as the preeminent leader of the new informational economy. In fact, Japan lags in the information age. According to Bill Baumol and his associates (*Productivity and American Leadership*), both Japan and Europe have a smaller share of their labor force in the new information and service sectors than does the United States (57% and 59%, respectively, compared to 71% in the United States). In addition, the United States is more competitive in the information economy. Recent studies by McKinsey, the international management consulting firm, show that American manufacturing and service industries have restructured well ahead of Japan and Europe and today enjoy a substantial lead in the information sectors that is, telecommunications, software, computer networking, multimedia technologies, and financial services. Ignoring these data, the authors also give no hint of the fundamental structural problems faced in Japan (and Europe). Recently, Japanese investment and growth have slowed precipitously. In 1992, Japanese manufacturing productivity dropped 6.2% while that of the United States rose 4.2%.

The volume also overstates America's decline. It repeats the litany of criticism of U.S. policies in the 1980s but says nothing about the downsizing and productivity improvements in American industry in that decade or the powerful surge of American exports from 1986 to 1992. American manufacturing productivity grew at a rate of 3.5% per year in the 1980s, against a rate of 2.5% per year in the 1950s and 1960s, and American exports doubled over the five years prior to 1993, regaining market share around the world. There is much to criticize in American policies in the 1980s, but it is an overreaction to label these policies "a romantic reaction, a return to the simplicities and uncertainties of America's uplifting past" (p. 123).

The failure to the drawbacks of Japanese—or the benefits of American—policy choices leads to curious recommendations for European policy. Europe is counseled neither to deregulate aggressively as in "Europe 1992" or to exploit trade and investment opportunities with the low-wage countries of Eastern Europe and the former Soviet republics as the United States deregulated and shifted production to the Asian advancing nations. Instead, Europe is urged to maintain its welfare system, develop national champions (even though elsewhere the authors acknowledge this strategy has failed), and pursue a European-level response involving "substantial amounts of government action in order to protect and transform the structure of European industries in profound ways" (pp. 95, 143). No mention is made of Europe's near-catastrophic unemployment problem (12%), budget deficits in Europe that on average are larger than the U.S. deficit (5.1% compared to 4.5% in 1992), or wage and benefit costs in Europe that far exceed those in Japan and the United States and cripple European competitiveness, precisely in those sectors where former communist countries could supply European markets much more efficiently and equitably.

The essay on developing nations is more reflective.

Cardoso, who did much to popularize the role of the state and dependency in North–South relations, now concedes that "politics is not the center of all change" and that "the theory of exploitation (of classes and of nations) has been shaken by an earthquake of magnitude 8 on the Richter scale" (pp. 151–52). Contrary to dependency logic, much of the South did develop; and it did so, according to Cardoso, not because of "more" or "less" state intervention but because of "better" state policies reflecting the "new rationality" of "decentralized forms of management and decisionmaking" (p. 155). Unless poor developing nations adopt the same policies, Cardoso fears they face an even crueler fate of marginalization and irrelevance. Unwilling to jettison his socialist preferences yet ambivalent about centralized policies, Cardoso calls for a democracy of "collective humanism" that creates "public spaces" and "a plurality of decisionmaking levels, making feasible a 'polyarchical utopia'," whatever that may be (p. 155).

Most of the essays were completed before 1992. An epilogue is therefore added that points to Clinton's election, the rise of nationalism in the former communist world, and the grass roots, populist revolt against the Maastricht Treaty as proof of the book's thesis that national policies are essential to rein in the individualist and laissez-faire tendencies of global economic liberalism. "The source of Clinton's landslide election," the authors conclude, "was his program to revive the American economy, with emphasis on *American*" (p. 162). While *landslide* is apparently meant to include the Ross Perot vote, the attempt to validate scholarly analysis with electoral results only confirms the one-sidedness of this study.

George Washington University HENRY R. NAU

International Peacekeeping. By Paul F. Diehl. Baltimore: Johns Hopkins University Press, 1993. 211p. $36.50.

Alas for authors in the field of international relations in the mid-1990s and, above all, for those who write about something as contemporary as United Nations peacekeeping! *International Peacekeeping* evidently went to press just before the "three cases from hell"—Bosnia, Somalia and Haiti—hit the political fan. This is a shame, because it might have enlivened a rather pedestrian, albeit comprehensive, survey of the universe of multilateral peacekeeping. The experiences of UNEF I and II, UNFICYP in Cyprus, UNOFIL in southern Lebanon, and others are all well known and not particularly controversial, to the point where a new truce-supervising peacekeeping venture is a dog-bites-man event.

But since 1991, the United Nations has ventured further into an unchartered and distinctly controversial realm, variously called "peacemaking," "humanitarian intervention," and (in Secretary General Boutros Boutros-Ghali's word) "peace enforcement." It began promisingly in northern Iraq after the Gulf War ended, with a still-resilient Saddam Hussein now battering his Kurdish population in the north and Shi'ites in the south. Public indignation drove the United States and United Nations (an inseparable pair for this purpose) to help those hapless souls despite the opposition of the Iraqi government—a first for peacekeepers. The breakup of Yugoslavia raised similar humanitarian concerns, as did anarchy and famine in Somalia and trashing of democratic government in Haiti. But Bosnia revealed the depth of Europe's moral insufficiency, Somalia and Haiti the superficiality of American's new commitment to multilateralism. It was not just man-bites-dog but watch-everyone's-hair-catch-fire in New York and Washington. The upshot was a jamming on of the multilateral brakes and an agonizing reappraisal of U.S. policy.

The present author missed all of this through no fault of his own. But his rather plodding approach to traditional peacekeeping (one of history's genuine political–military innovations) suggests that even an updated version of the book might not stimulate the student or scholar to pursue it further. That is the bad news. The good news is that for an inventory of names and numbers of the players, along with some references to relevant political doctrines that are buried in the text, this book is probably the most recent on the topic.

Massachusetts Institute of Technology LINCOLN P. BLOOMFIELD

Bridging the Gap: Theory and Practice in Foreign Policy. By Alexander L. George. Washington: United States Institute of Peace Press, 1993. 170p. $24.95 cloth, $14.95 paper.

Alexander George undertakes an ambitious task in *Bridging the Gap* and does a more-than-creditable job in accomplishing it. George sets out to "encourage better communication and closer collaboration between academic scholars who study foreign policy and practitioners who conduct it." After describing the disparate cultures of academia and policy-making, George gets to the heart of his argument by identifying three types of knowledge that can help policymakers decide whether and how to employ a particular strategy: abstract, conceptual models of each strategy; generic knowledge of the conditions that impinge upon the likelihood of success of a strategy; and actor-specific, behavioral models of the adversary. George carefully analyzes U.S. policy toward Iraq from 1988 through 1991 and concludes that there was an inadequate knowledge base for that policy. In the book's concluding chapter, he offers six implications for scholarly research and policymaking.

Bridging the Gap does an excellent job of diagnosis. The author's substantial experience in both the academic and government policy-making worlds combines with his insight to allow him to present a cogent analysis of the differences between the cultures of academia and government policy-making. Both academics and policymakers should find themselves nodding in agreement with George's observations about the two cultures. The "gap" may or may not be bridgeable but the necessary first step in attempting such a task is the type of carefully thought-out diagnosis of the problem that George provides.

The three types of knowledge that George puts forth as essential for helping policymakers decide whether and how to employ a particular strategy are all quite important. I find the call for such models to be highly insightful, but I do urge an embellishment of that call. In order to understand and assess virtually any set of circumstances in foreign policy (including the recent U.S. policy toward Iraq that George uses as his primary example), one must analyze more than dyadic interactions. It is important both that such actor-specific behavioral models include the adversary's view of other actors and that they be developed for more actors than just the acknowledged adversary. These embellishments would substantially enhance the policy utility of such models.

George understands that *theory* can be a pejorative term in the policy-making community. For that reason, he employs the term *generic knowledge* when arguing for the relevance of theory. My first reaction is sadness at George's undoubtedly correct analysis of the potential response of many to the term *theory*. However, I am buoyed by the emphasis, within that discussion, on what one might term scope conditions for generalizability. Such scope conditions are clearly a critical part of any bridge between theory and practice. The plea for such scope conditions is one that, if heeded, would also enhance communications between a variety of academic communities. Also, George is to be commended both for addressing some of the shortcomings of applying a strictly neorealist paradigm to foreign policy questions and for highlighting research that more carefully analyzes the theory of appeasement than any other post–World War II author of whom I am aware. George attempts to apply evidence from more cases than Nevelle Chamberlain's ill-fated policy toward Hitler in drawing his conclusions.

As George undoubtedly appreciates, he is the latest in a long line of scholars attempting to bridge the gap between theory and practice in foreign policy. In much of *Bridging the Gap,* I am reminded of Davis Bobrow's "Relevance Potential of Different Products" (*World Politics* [1969]). Among other similarities, both argue for the relevance of theory, the importance of bridging the gap, and then offer a "construction plan" for the bridge. However, where Bobrow concentrated more on the importance of focusing on policy-manipulable variables (also mentioned by George) and offered only a variety of brief examples, George's use of a set of examples all drawn from U.S. policy toward Iraq communicates well to readers in both academic and policymaking communities. Emphasizing the relevance of the proposals that George is making by applying them to such a recent foreign policy case will certainly broaden the audience that pays attention to his message.

Building upon the message of George's book would be an important next step. In his concluding chapter, he provides a number of possible avenues for accomplishing this. I will now add to that list. A more specific and comprehensive application of the types of knowledge for which he calls is crucial. Either recent U.S. policy toward Iraq or another case should be employed even more comprehensively to show exactly how the types of knowledge for which George calls could be employed to produce improved policy results over failure to employ such knowledge. George outlines what models employing the three types of knowledge should contain. Building such models based on "conditional generalizations" and interacting with policymakers about them might start the type of dialogue that George is seeking.

Ohio State University DONALD SYLVAN

Minorities at Risk: A Global View of Ethnopolitical Conflicts. By Ted Robert Gurr. Washington: U.S. Institute of Peace Press, 1993. 448p. $37.50 cloth, $24.95 paper.

Ted Robert Gurr is probably best known for his *Why Men Rebel* (1970) which remains the fullest application of relative deprivation theory to conflict analysis. In *Minorities at Risk,* Gurr surveys the world to present "an integrated substantive and empirical analysis of communal status and conflict since the end of World War II, with special attention to the decade of the 1980's" (p. xi). This time, Gurr steers a median course between relative deprivation and group mobilization explanations of conflict and also between primordialist and instrumentalist interpretations of ethnicity. In this reasonable trade-off, theoretical incisiveness is sacrificed to a looser analysis that better accommodates a multitude of cases.

The empirical core of the book is a comparative, cross-national analysis of 233 politically active communal groups (whose members number some 915 million persons) in 93 countries. These groups are coded into five categories: *ethnonationalists,* large groups with a history and dream of political autonomy (e.g. Croats, Québecois); *indigenous peoples,* conquered descendants of original inhabitants (Miskitos, Maoris); *ethnoclasses,* low-status minorities descended from slaves or immigrants (African–Americans, Koreans in Japan); *militant sects,* communities politically defined by religion (Northern Irish Catholics, Lebanese Maronites); and *communal contenders,* culturally distinct groups seeking to retain or augment their share of the national pie, who may be subcoded as *advantaged* or *disadvantaged* (Tutsi of Burundi, Chinese of Malaysia, respectively). The boundaries of both groups and categories are admittedly "fuzzy," and several groups (notably, all but 8 of the 43 militant sects) are coded under more than one category.

Specialists will properly quibble about coding and terminology: Why should the Masai be more "indigenous" than the other peoples of Kenya and Tanzania? Is it not at least a misnomer to apply the label "militant" to the Baha'is of Iran? Still, the categories are firm enough to yield useful empirical statements when plotted against indices of cultural differences, discrimination, and types and frequency of political action. As Gurr shows, overall, every form of ethnopolitical conflict has increased since the 1950s, but trends differ widely among regions. For example, within Western democracies, conflict peaked sharply in the 1970s, then declined, while in Eastern Europe, it increased sharply in the 1980s. Cultural differences have no effect on inequalities for ethnoclasses or communal contenders, but they are associated with discrimination against ethnonationalists. In general, cultural differences have a greater effect on economic than on political disadvantage. Religious cleavages are seldom at the bottom of communal conflicts, though they may help exacerbate such conflicts. While Gurr's categories are not ideally set up for this purpose, little in his findings would provide empirical comfort for a Huntingtonian vision of a world about to be riven by a "clash of civilizations."

Four of the book's 11 chapters provide regional context and exemplification for the global generalizations. Monty G. Marshall deals with Eastern Europe and the former USSR, Barbara Harff with North Africa and the Middle East, and James R. Scarritt with sub-Saharan Africa, while Gurr himself takes on the Western democracies and Japan. Useful as these chapters are, none has the space to provide the detail and the qualifications one would like. The sub-Saharan Africa chapter strikes the most graceful balance between reverence for the comparative framework and the specific demands of local experience, though too much weight is placed on the colonial experience in accounting for the ethnic diversity of African states. The East European chapter spends disproportionate space on discussing the differences between Lenin's and Stalin's views of the "national question." The North Africa and Middle East chapter's shaky discussion of Berber-Arab relations in Morocco should be bolstered by a more systematic comparison with such relations in Algeria. And who let slip by the howler assigning the Turkish language an "Indo-Germanic origin?" (p. 221).

Although *Minorities at Risk* does not provide a major conceptual or methodological breakthrough, it provides a wealth of useful empirical material and a model of large-scale cross-national comparative work on complex and important subject matter. The book should quickly find its way onto the required reading list for courses on nationalism and ethnic conflict, where it would provide stimulating intellectual counterpoint for more speculative exercises of historical interpretation like Anderson's *Imagined Communities* (1983) and Moynihan's *Pandaemonium* (1993). It also should provide those immersed in regional studies with a comparative context for their more parochial discussions of communal conflict. In an era when support for large-scale empirical work is hard to come by, the U.S. Institute of Peace is to be congratulated for having sponsored and sustained this ambitious project.

Yale University WILLIAM J. FOLTZ

The Politics of Economic Adjustment. Edited by Stephan Haggard and Robert R. Kaufman. Princeton: Princeton University Press, 1992. 356p. $49.50 cloth, $16.95, paper.

Since the early 1980s, the challenge of adjustment has dominated economic policy throughout the developing world. In many countries, economic reform has been concurrent with democratization and regime change. Neoclassical reform measures, urged upon indebted countries by multilateral lenders and G-7 governments, pose significant challenges to the sovereignty, ideology and domestic prerogatives of state elites. Stabilization and adjustment programs demand substantial administrative capacity and political skill, while the powerful distributive conflicts unleashed by economic change threaten the legitimacy and stability of incumbent regimes. Advocates of neoclassical reform suggest its compatibility with democratization, yet the relationship between political and economic liberalization remains ambiguous.

The Politics of Economic Adjustment represents an impressive effort to summarize the political legacy of economic reform, and to clarify the elusive relationship between political transition and economic change. In its comparative breadth and theoretical depth, this volume has few peers in the contemporary literature in comparative political economy. The chapters offer important contributions to current debates on economic change and regime transition, and the book provides a wealth of policy-relevant insights which might also be applied to the transition economies of Central and Eastern Europe.

The Politics of Economic Adjustment is the third volume in a series of studies on the politics of economic reform. The companion works, *Fragile Coalitions: The Politics of Economic Adjustment* (1989) and *Economic Crisis and Policy Choice* (1990), both edited by Joan Nelson, were respectively devoted to the policy dynamics of reform and a series of comparative case studies of economic adjustment in Latin America, Africa and Asia. The volume under review seeks to synthesize the conclusions of the earlier efforts, and to offer a number of middle-range propositions about the political context of adjustment. The book's seven extended chapters are organized around four broad themes: the relative salience of international influences and domestic actors in the process of economic adjustment; the role of the state in economic restructuring and the scheme of state reform; the distributive implications of stabilization and adjustment strategies; and the relationship of economic adjustment and democracy.

These are controversial issues, and the analyses in this volume offer critical revisions of many common premises regarding politics and economic reform. One such notion is that orthodox programs have been successfully imposed by a monolithic creditors' cartel, led by the IMF and the World Bank. In the opening section, Barbara Stallings and Miles Kahler provide discerning evaluations of the international context of economic adjustment. Stallings considers the distinct roles of international markets, policy networks and northern-dominated regimes in promoting reform among developing countries. While external structural constraints and transnational networks provided complementary channels influencing an initial shift toward orthodox adjustment, Stallings notes that international factors are less compelling in accounting for policy implementation and outcomes.

Echoing this caveat, Miles Kahler acknowledges the importance of international factors in agenda-setting, but observes that international financial institutions and northern commercial banks had relatively weak leverage over wide realms of debtor-country policy choice and implementation. In fact, the global regime governing debt and adjustment weakened throughout the 1980s, as the "conditionality bargain" offered by creditors failed to offer substantial incentives for imposing politically costly austerity measures. Eventually non-conditional resources were renewed, and the unity of creditor groups frayed as they pursued divergent strategies. Many developing countries were able to evade conditionality or to pursue heterodox strategies.

The limits of international-level explanations point to a greater stress on the domestic politics of reform, and succeeding chapters offer both state-level and society-centered analyses. A second perspective challenged in this volume is the neoclassical assumption that state intervention is inherently detrimental to economic growth. In the book's strongest theoretical chapter, Peter Evans reassesses debates about the developmental state. States confront basic collective action problems, observes Evans, and neither arguments about the rent-seeking "predatory" state, nor depictions of a coherent, autonomous "developmental" regime adequately capture the political qualities leading to economic success. Evans offers the compelling notion of "embedded autonomy" to portray the character of developmental states. Such regimes embody internal coherence, discipline and autonomy from fragmentary societal interests, yet they also maintain diverse linkages with society through personal networks and corporatist arrangements, which serve to garner information and foster compliance.

John Waterbury's comparative analysis of public sector reform in Egypt, India, Mexico, and Turkey provides a laudable attempt to portray the scheme of state reform in more empirical fashion. Waterbury notes that heavily statist economies give rise to similar forms of property rights and interest group coalitions, implying comparablity in the dynamics of reform. He emphasizes the importance of technocratic "change teams" within the state, endowed with a mandate to implement a reformist project. Impelled by growing fiscal constraints, reform-minded state elites may surmount the scattered opposition of a statist coalition, and privatize or restructure state enterprises.

A third controversy confronted here is the impact of stabilization and adjustment on the poor, and effects of distributive tensions on social conflict and political stability. Joan Nelson analyses discrepancies between the differential costs of adjustment and the relative political salience of affected groups. While urban, formal sector groups experience greater losses under adjustment, the urban and rural poor are more vulnerable, since they possess fewer strategies for coping with price changes and income reductions. However, privileged urban constituencies exercise preeminent political influence, and are likely to block redistributive policies which could reduce the vulnerability of the very poor. Nonetheless, organized labor and urban dwellers are more likely to engage in sporadic, disparate protests against particular effects of reform rather than concerted protest against adjustment per se. Government strategies for dealing with affected groups, and general popular perceptions about economic management, may significantly assuage protest and opposition.

In a comparative study of the politics of inflation and stabilization, Stephan Haggard and Robert Kaufman also stress regime factors rather than the strength of distributive coalitions. They find that inflationary tendencies are attributable to the relative security of elite tenure and the capacity of political institutions to resist distributive pressures. While recognizing the contribution of external shocks and devaluations to inflation, they emphasize that rates of inflation and proclivities toward stabilization are more closely related to regime type. They also observe that authoritarian governments evince few generic advantages over democracies in pursuing stabilization, although relatively cohesive authoritarian governments reflect a stronger record of limiting high inflations.

The focus on distributive pressures and regime response raises the central issue of political transition. Is democracy compatible with economic adjustment? Or, do the political costs of adjustment weaken transitional governments and foster authoritarian management? Haggard and Kaufman conclude with a chapter assessing the impact of economic crisis on regime transition. They refute the impression that economic decline in the 1980s prompted extensive regime change, and they observe that weak economic performance has not produced a widespread breakdown in transitional democracies since the early 1980s. They relate the stability of regimes, both authoritarian and democratic, to relative levels of elite cohesion and political institutionalization. The sequencing of political and economic change, and the configuration of executives and party structures, are crucial determinants of democratic transition and consolidation.

There are few qualms one might raise about this collection. While the volume generally avoids the eclecticism of many edited works, the authors naturally hold divergent views over central questions. Certain authors seem too ready to accept the orthodox agenda as a framework for evaluating policy choice and performance, even developments prior to the 1980s. Moreover, the comparative analyses sometimes tend toward a welter of idiosyncratic qualifications, thus blurring the strengths of the comparative framework and obscuring important conclusions. These are largely stylistic weaknesses, however, and they should not detract from the commendable strengths of this book. *The Politics of Economic Adjustment* represents the most comprehensive comparative work in this area, and it is a collection of impressive analytical rigor and theoretical depth. It will provide a lasting touchstone for the study of economic and political reform.

American University PETER M. LEWIS

The United States and Latin America in the 1990s: Beyond the Cold War. Edited by Jonathan Hartlyn, Lars Schoultz, and Augusto Varas. Chapel Hill: University of North Carolina Press, 1992. 328p. $39.95 cloth, $15.95 paper.

The end of the Cold War marked a turning-point in inter-American relations. From the early nineteenth century through the late 1980s, the U.S. policy toward Latin America primarily focused on real, potential, or imagined threats to its national security from extrahemispheric powers operating in or through Latin American states. In particular, for much of the period from the late 1940s through the 1980s, U.S. decisionmakers molded military, economic, human rights, immigration, and other policies to counter the strategic and ideological challenge posed by the Soviet Union. Superpower rivalry strongly influenced both the foreign policies and the domestic politics of many Latin American countries. Thus, the end of the Cold War, in combination with important political changes in the United States and in major Latin American countries creates new opportunities for U.S.–Latin American cooperation.

This outstanding collection of essays examines key trends in U.S.–Latin American relations and the consequences of recent international and domestic political developments for hemispheric affairs. The editors and the 12 Latin American and U.S. contributors are among the foremost authorities in their fields. Together, their essays provide a comprehensive examination of both unresolved tensions in inter-American relations and the specific problems facing U.S. and Latin American policymakers in the 1990s.

The book begins with the editors' introductory essay and three overview chapters by Alberto van Klaveren, Augusto Varas, and Abraham F. Lowenthal. The authors of these essays concur that the end of U.S.–Soviet strategic rivalry in the Western Hemisphere, global economic restructuring, the democratization of foreign policymaking in the United States, and a widening consensus in major Latin American countries in support of political democracy and market-led economic development have significantly transformed U.S.–Latin American relations. Among other things, these changes have further eroded old foreign policy assumptions and produced greater heterogeneity in the bilateral foreign policies of hemispheric states.

Van Klaveren, for example, discusses Latin American countries' efforts since the 1970s to strengthen diplomatic and economic ties with West European countries and Japan, as well as the prospects for collective Latin American action on economic and security problems. Varas's chapter examines shifts over time between "hegemonic" and "coercive" paradigms of hemispheric order, noting that the coincidence of particularly favorable international and domestic conditions may lay the basis for "cooperation through partnership" in the 1990s. Varas argues that the "coercive continuities" in U.S. policy (most recently manifested in the invasion of Panama in December 1989 and the assumption that we retain the *right* to intervene in the affairs of Latin American states to protect its interests) can most effectively be constrained—and bipartisan consensus promoted—by issue-specific security regimes that reduce confusion between U.S. political goals and Latin American countries' security concerns. Lowenthal's essay convincingly demonstrates why, even after the disappearance of "the Soviet threat" in the Western Hemisphere, Latin America remains of vital importance to the United States. Among other reasons, hemispheric states are bound by issue agenda (migration, drug trafficking, environmental problems) that increasingly link different countries' domestic and foreign policies.

The introductory essays offer both useful historical background on inter-American political and economic relations and an assessment of the specific challenges that will confront Latin American and U.S. policymakers during the 1990s. However, they could have made an even stronger contribution by identifying more clearly the general principles that should guide U.S. and Latin American foreign policymakers now that the Cold War is over. The question whether existing inter-American institutions are capable of promoting cooperation and limiting conflict in this new era also merits further attention.

The second part of this volume consists of essays examining domestic political trends in Latin America and specific policy areas in inter-American relations. The chapters by Rosario Espinal and Marcelo Cavarozzi analyze, in Latin America the evolution of the political Right and Left, respectively. Attention to changes in Latin American countries' domestic political situation is especially welcome in a book on U.S.–Latin American relations, and these essays offer far-ranging assessments of recent developments on the Right and the Left. Cavarozzi is, moreover, particularly effective at capturing the interplay between international conditions and domestic political change in the region since the 1930s.

Other authors assess the evolution of the Latin American debt problem in the 1980s (Riordan Roett), U.S.–Latin American trade relations (Roberto Bouzas), the issue of military involvement in Latin American politics and U.S. policies concerning democracy and human rights (J. Samuel Fitch), U.S. and Latin American policies on drug trafficking (Bruce Bagley and Juan Tokatlian), environmental problems (Steven Sanderson), and migration issues (Robert Bach). In addition to providing comprehensive analyses of the main issues in these different areas, these chapters identify many of the unresolved dilemmas that will confront U.S. and Latin American decision makers during this decade. Bagley and Tokatlian, for example, show that despite a growing regional consensus in the 1980s that drug smuggling constitutes a major societal and national security problem, U.S. policy has lacked legitimacy and credibility because it is based on an inappropriate conceptual definition of the problem and a counterproductive U.S. preference for unilateral, often military, actions to resolve it. Bach demonstrates how domestic politics has influenced U.S. immigration policies over time, how these policies and other U.S. foreign policy goals are linked, and how U.S. policies have shaped migratory flows from Latin America.

In sum, the editors and contributors succeed admirably in assessing the foreign policy consequences of major changes in the international arena and in the domestic politics of hemispheric states, new sources of complexity in U.S.–Latin American relations, and potential sources of future cooperation and conflict. The book is a significant addition to the scholarly literature on inter-American relations. It is also an important resource for graduate and undergraduate teaching.

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KEVIN J. MIDDLEBROOK

The New Cold War? Religious Nationalism Confronts the Secular State. By Mark Juergensmeyer. Berkeley: University of California Press, 1993. 292p. $25.00.

This is an interesting book on a topic of contemporary concern. The rise of various ethnic and religious movements, which are challenging traditional authorities in many countries of the Second and Third Worlds, has led to the fear that the West faces a new confrontation with various forms of religious fundamentalism. The author does not like the term *fundamentalism* because it is pejorative, "is an imprecise category for making comparisons across cultures," and does not carry any political meaning (p. 5). Instead, he contrasts what he prefers to call religious nationalism with secular nationalism and, in a series of chapters, shows how the former has challenged the latter in a number of countries in the Middle East, South Asia, and the formerly communist countries in Central Asia and Eastern Europe. Then, in successive chapters, he addresses the questions, Why does religious nationalism turn violent? Is a religious state compatible with democracy and the maintenance of human rights? Can the West live with religious nationalism?

The author sees the rise of religious nationalism as a reaction to the perceived failure of secular nationalism to deliver on "its promises of political freedom, economic prosperity, and social justice" and to the moral corruption secularism is claimed to have engendered (p. 23). He attempts to set the conflict between secular and religious nationalism in the perspective of the history of ideas, as well as Western history. This is the least satisfactory part of the book and seems jejune by contrast with the discussions of nationalism by Berlin in *Against the Current*, and Plamenatz in Kamenka's collection, *Nationalism*, and with the different strands of thought that have contended historically in the formation of the European state discussed by Oakeshott in his *On Human Conduct,* and, more recently, his Harvard Lectures, *Morality and Politics in Modern Europe.*

The most interesting chapter is the one in which he examines the reasons for violent religious confrontations and argues: "One can think of religious warfare as a blend of sacrifice and martyrdom . . . But behind the gruesome litany is an idea that encompasses both: . . . the dichotomy between the sacred and profane. This great encounter between cosmic forces—an ultimate good and evil, a divine truth and falsehood—is a war that worldly struggles only mimic" (p. 155). He also argues that the modern religious state need be neither antidemocratic nor against "the minimum definition of human rights—the notion that people should be able to reside peacefully alongside each other in dignity and with personal security" (p. 186). He makes a distinction between modernity, which religious nationalists are happy to embrace, and modernism ("the ideology of individualism and a relativist view of moral values," p. 191), which they are not. The religious nationalists are unlikely to support the "libertarian version of individual rights. . . That difference is so deep and abiding that it will not be easily resolved" (p. 189). But ultimately he thinks religion and reason, even if embodied in different nation-states, can coexist, and hence does not foresee any real danger of a new Cold War.

The main competitor of this volume is the second volume of the monumental study of fundamentalism organized by the American Academy of Arts and Sciences (AAAS), Marty and Appelby's collection, *Fundamentalism and the State.* Though the volume under review does have the merit of the greater coherence of a volume written by a single hand, it loses somewhat in terms of the richness of the AAAS volume. Also, it misses some important nuances. For instance, an important theme in the politics part of the AAAS volume is a distinction between the sectarian state and the nation-state, which leads to a finding that religious fundamentalists are much more likely to be successful in capturing the former. Moreover, the author virtually ignores the economic failures and successes that have distinguished states where fundamentalists (a term I still prefer to *nationalists*) have been resurgent from those where they have not. For instance, why have East Asia and the more successful countries in Latin America not seen a rise in religious fundamentalism? There is a storyline that can tie up all these loose ends. But even if Jurgensmeyer has not written the definitive book on religious fundamentalism, it is good to have some of the hysteria that has surrounded the subject in official circles and the media effectively dispelled.

University of California, Los Angeles Deepak Lal

Protecting Markets: U.S. Policy and the World Grain Trade. By Ronald T. Libby. Ithaca: Cornell University Press, 1992. 152p. $26.50.

Regulating Unfair Trade. By Pietro S. Nivola. Washington: Brookings Institution, 1993. 190p. $31.95 cloth, $12.95 paper.

These two short books provide considered, scholarly examinations of U.S. efforts during the 1980s to counteract objectionable trading practices of other countries and ensure a "level playing field" for U.S. firms. Although both hedge their bets, their conclusions are at odds.

The Nivola volume is broader and more comprehensive and carries its analysis closer to the present. It provides a partial answer to a key question: If trade negotiations under the GATT were so successful, why is the world slipping toward increased protectionism and perhaps even a trade war? Nivola's basic thrust is that trade regulation is being asked to do to much. Policymakers are unlikely to be able to fix broad macroeconomic failings simply by stiffening U.S. trade regulations. In effect, Nivola urges that trade policy actions ought to be aimed at remedying trade problems. He offers policy suggestions that the United States might adopt to refocus attention on open markets in reality, not just in rhetoric.

Sequential rounds of GATT trade negotiations made it politically unfashionable to maintain high tariff barriers. So to protect and encourage domestic industries, many countries started relying more on nontariff barriers. Frustrated, U.S. legislators demanded that in the name of equity, fair trade take precedence over free trade as the central U.S. trade objective. Their challenge was to help their beleaguered constituents without resorting to overt protectionism. To accomplish this task, the United States filed a growing number of antidumping suits and countervailing duties, negotiated many new orderly marketing arrangements and voluntary export restraint programs, and experimented with new approaches such as the Super 301 program and the Structural Impediments Initiative.

Legislators did not want to be portrayed as protectionists, so they instead tried to aid firms and workers injured by unfair competition. Nivola reviews both congressional and presidential initiatives. On the legislative side, the march from the watershed Trade Act of 1974, to the Trade Act of 1979, the Trade and Tariff Act of 1984, and (finally) the Omnibus Trade and Competitiveness Act of 1988 are summarized. At the same time, the increasingly activist intervention by the executive branch (sometimes in collaboration with Congress) is described. Still, by the mid-1980s it was clear that the international competitiveness of the U.S. automobile, steel, footwear, and textile industries was still poor.

What is largely absent from this analysis is a serious examination of the areas in which the United States experimented with new ways of encouraging freer trade, particularly with regard to so-called new issues and in smaller forums. As this volume makes abundantly clear, there is no such thing as a coherent trade policy. Thus, greater attention to U.S. efforts to liberalize trade in services and develop new understandings covering the protection of intellectual property and the handling of trade-related investment measures would have complemented the analysis. And consideration of the efforts by the Reagan, Bush, and Clinton administrations to achieve free-trade agreements with Israel, Canada, and Mexico are absent.

Still, there is an elegant power in Nivola's argument that builds with simplicity, logic, balance, and ultimately deliberateness and reasonableness. Nivola sets out to make his point and moves step by step through his argument. Nothing is rash, nothing is surprising. There are no bombshells as the weight of the argument builds steadily with each chapter. Nivola shows how the U.S. executive and legislative branches set about to remedy the perception of foreign unfairness and bolster flagging

American confidence (most often without requiring U.S. firms to correct their wayward habits). One result was a proliferation of moves to try to remedy unfair trade practices. Another result was the politicizing of U.S. trade policy. Indeed, the full-court political press, in the light of tremendous public scrutiny, that was necessary to persuade Congress to ratify the North American Free Trade Agreement is a perfect illustration. Until recently, such an agreement would have passed Congress easily without fanfare.

The recommendations offered by Nivola are modest and sensible, if unsurprising. He dismisses the idea that the United States should simply abandon efforts against unfair trade as politically naive and unrealistic. But he worries that crafting a consistently successful strategic trade policy is beyond the ability of government officials. He concludes rather tamely that the United States needs less dead-end moralism and that it should go back to basics. It "ought to keep pressing for equitable rules of commerce abroad but also carry out economic changes needed to make government a better partner of industry at home" (p. 142).

By contrast, Ronald Libby's *Protecting Markets* is a narrow case study that examines whether the Export Enhancement Program (EEP) adopted by the Reagan administration in 1985 to subsidize U.S. agricultural exports could be counted as a successful experiment in mercantilist trade policy. Libby states that the EEP was designed to restore U.S. grain markets that had been lost to unfairly subsidized European Community grain exports and to increase Europe's financial burden under its Common Agricultural Policy (CAP), thus diminishing its ability to engage in a subsidy war with the United States. The idea was to win back markets and to create such high levels of tensions within the European Community—particularly between the United Kingdom and France—that the Community would agree to negotiate reduced agricultural subsidies within the context of the Uruguay Round of multilateral trade negotiations in the GATT. Libby concludes that this goal was met—that Europe agreed to negotiate to reduce trade barriers under the pressure of discriminatory trade measures imposed by the United States.

This is a curious, slim volume with much to commend it, but its specific conclusions are ultimately unconvincing. This makes it hard to generalize from them. The book provides a careful, detailed analysis of political economy of U.S.–French (more than European) rivalry to capture cereal export markets for their producers. It shows how European (particularly, French) exporters aided by huge subsidies under the CAP displaced U.S. farm exports in the two decades preceding 1985. The interactions among France, Germany, and the United Kingdom with regard to agricultural trade are examined in detail.

In the 1980s, the United States started to act more forcefully to counter the loss of agricultural markets: "From the EEP's inception until October 1991, 119 export initiatives were approved by the USDA involving 80 countries and twelve commodities" (p. 53). The description and analysis of the continuing tussle between the United States and Europe in the mid-1980s, just before the designation of agriculture as a central agenda item of the Uruguay Round negotiations, is the best part of the book. Sadly, there is meager examination of the intricacies of negotiations on agriculture during the trade round itself.

Libby's claim that the EEP was an effective mercantile policy depends on whether or not you are a U.S. agricultural exporter. The United States did at least temporarily regain some markets for U.S. exporters, in part because of the EEP; but American taxpayers footed the bill for almost $3.8 billion from 1985 to late 1991. Ironically, Cargill, the largest single U.S. beneficiary, consistently opposed the program but was nonetheless awarded nearly $700 million in bonuses between 1985 and 1991. Some citydwellers might consider this a high price to pay to U.S. agricultural interests just to get the European Community to talk about agricultural subsidies, particularly if larger supplies in the United States might have kept prices lower. Moreover, farmers could well conclude that it was better to keep the subsidies provided under the EEP than to try to compete in an unsubsidized world. Even the Blair House compromise, which may help conclude the Uruguay Round talks (which took place after this book was completed), involves only small concessions by the French and ones that they would like to revisit.

Indeed, Libby undercuts the life of his own case for mercantilist intervention when he cautiously (and probably correctly) concludes, "Although the EEP was effective as a diplomatic bargaining tool only when U.S. grain supplies were large, world prices low, competition keen, the value of the U.S. dollar low, and most crucially political tension over the burgeoning CAP budget high, a U.S. recovery from the drought and renewed political tensions in the EC undoubtedly will make the Export Enhancement Program an effective lever in future GATT negotiations" (p. 130). That is a thin reed on which to advocate renewed mercantilism. Overall, score one for Nivola.

University of Southern California JONATHAN D. ARONSON

Gendered States: Feminist (Re)Visions of International Relations Theory. Edited by V. Spike Peterson. Boulder: Lynne Rienner, 1992. 225p. $32.00 cloth, $16.95 paper.

Gendered States provides an important critique of international relations theory by problematizing one of the theory's most central and privileged concepts, the state. The book provides critical analysis and deconstruction of the relationship between gender and a number of global issues, including security, sovereignty, and revolution. The contributors uncover the centrality of the too-little-questioned conceptualization of the state to traditional international relations theory and unmask the gendered privileges that states accord select men. Many problems associated with international-relations theory's conceptualization of the state—including how the state oppresses, degrades, and even coopts women to maintain and reproduce itself and expand its will around the world—are highlighted. The contributors, mostly feminist international-relations scholars, argue that a feminist analysis of the state and international-relations theory is central to an understanding and recreation of the world system. Consequently, the authors attempt to go beyond critique by furnishing a discussion of what they envision as significant aspects of a feminist international-relations theory.

The volume's strength lies in its analysis of the impact of the gendered nature of state formation, maintenance, and reproduction. This is done through three analyses: one focuses on the parallels between early Athenian and modern state formation, highlighting the differential roles (and corresponding power positions) accorded to men and women; one, on the continual oppression of women after modern revolutions; and one, on the lack of a feminist consciousness in the testimonies of contemporary military women coopted into the masculine culture of military service. These analyses convincingly show the state's historical and continued dependence on gender differentiations and the centrality of women and women's roles to global politics.

While the chapters make strong arguments for the importance of "taking gender seriously," the volume itself fails to take race, class, culture, sexuality, and other intragender differences seriously. Instead, the contributors privilege the history and experiences of select European women; and most chapters fail to include any serious analysis of the experiences of women of diverse backgrounds within and across states. At the same time that the volume challenges the masculinist account of political identity, autonomy, and community grounded in international relations theory, it replaces that account with accounts primarily based on the experiences of historically privileged European women. Most of the contributors assume—and at times suggest—that the developmental logic of the state and its corresponding subjugation are the same for all women, people of color, people of the lower classes, and people with alternative sexualities. This assumption results in the denial of differences in the oppression experiences of diverse peoples and obfuscates the role that states may accord some privileged women in the oppression of nonprivileged women, people of color, and poor people. The volume perpetuates the myth of the comprehensiveness of Western feminist analysis—similar to the myth of the comprehensiveness of realist analysis in international-relations theory.

The volume raises questions whose answers may lead to a feminist revision of international-relations theory. The contributors (re)vision goes beyond simply adding women to the study of international relations. It contests many of the theory's foundations—including theoretical justifications for, and the practice of, utilitarian autonomy—and argues, instead, for "relational autonomy" between parties in the conduct of world politics. It argues for selectivity, by the exclusion of coopted female experiences, in the collection of women experiences on which to build its theory. It argues for the acknowledgement and analysis of the interconnections of gender, family, states, and revolution. It argues for the ecofeminist lenses of "fractious holism" that reject the appropriation and exploitation of women and nature. It argues for the rejection of notions of political identity as sacrifice. Most intriguingly, it argues for the maintenance of a much-revised liberal state where difference is not shunned and minorities are accorded political voice.

Even with these revisions, one wonders what a feminist international-relations theory inclusive of race, class, and sexuality would entail. Clearly, such a theory will have not only to uncover the multifaceted array of privilege, power, and hierarchy within and across states but also to explain how these oppressive structures of the world system can be dismantled and changed. *Gendered States* provides a sneak preview of what the underlying basis of a revised theory of international relations should include. What the field may well need, however, is a *new,* not a revised, theory of international relations. In a time when very few of the immense global problems facing the world's peoples (over 50% of which are a diverse group of women) have gone unanticipated by international-relations scholars, movements to refine and redefine international-relations theory are necessary. *Gendered States* provides an initial blueprint of the kinds of questions a new theory of international relations must answer if we are to make any sense of the global village we live in.

Purdue University PATRICIA T. MORRIS

Waiting for the Millennium: The United Nations and the Future of World Order. By J. Martin Rochester. Columbia: University of South Carolina Press, 1993. 347p. $39.95.

There are several paradoxes associated with the United Nations. The organization has always been the object of contradictory attitudes: at the same time that many pin their hopes for world peace and security on the United Nations, others discard it as irrelevant and superfluous or even harmful and dysfunctional. Whereas summary judgments about the merits or demerits of the United Nations tend to be based on the organization's activity in the peace and security field, three-fourths of its resources are used in economic, social, and technical areas. Moreover, there is a wide gap between expectations and resources: the organization that is supposed to be the guarantor of peace and world order has an annual regular budget smaller than that of the New York City Police Department.

While noting such contradictions, Martin Rochester's study proceeds from another paradox. At a time when profound global changes have revived optimism about the future of the United Nations, international-relations theory is pervaded by an unprecedented skepticism toward the prospects of international organization. The regime literature is the specific target of Rochester's dispute with contemporary scholarship. His criticism is sometimes overstated, and his claim that scholars studying the United Nations have risked "pariah status in the international relations fraternity" is clearly exaggerated (p. 245). At any rate, Rochester proffers an alternative *institutionalist* and *globalist* approach to international order.

Trying to steer a course between "bad idealism" and "bad realism," the author launches an inquiry into the growth potential of global organization as one component of the quest for world order. He urges us to think of international system transformation as *political development,* understood as the cultivation of a world order that combines stability with adaptability. Adopting Karl Deutsch's terminology, he envisions a progression toward a global, pluralistic security-community. Rochester argues that the prerequisites of global institution building—a systemwide crisis and a critical mass of actors with the will and power to move the system—are present today. The main problem, in his view, is not a power vacuum but a leadership vacuum.

Rochester's assessment of the UN system starts with an overview of prominent shortcomings, including the lack of coordination between the specialized agencies, the fact that the politics of agenda setting tends to be divorced from the rest of the policy process, and the discrepancy between egalitarian voting rules and power inequalities among UN member states. This is followed by a thorough account of various reform proposals through the years, which shows a pattern of recycled ideas skirting the edges of institutional change.

Finally, Rochester offers a series of prescriptions based on a model of "dirigible pluralism" featuring subsystem autonomy within a central guidance system built around the United Nations. His reform proposals lean more toward "tinkering" than "rethinking" (to use the author's own terms). They can be realized with only marginal revisions of the UN Charter. First, Rochester recommends a number of measures to professionalize and depoliticize the UN Secretariat, including improvement of the secretary general selection process, more systematic use of competitive exams in recruiting personnel, centralization to a smaller number of superdepartments headed by Deputy Secretaries-General, and better intelligence capability through the creation of global-watch structures. Other prescriptions concern improved interagency coordination through the appointment of lead agencies and operations boards for specific functional area, budget process reform, and the restitution of ECOSOC to a pivotal role in bringing the programming and financing tracks closer together.

How, then, can such reforms be implemented, given the dismal track record of UN reform? Rochester envisions a trade-off—or enlightened, concert-of-powers approach—whereby major donors convert voluntary fundings into assessed funding in return for greater control over the budget process.

Waiting for the Millennium is an informative, cogently reasoned, and (in many respects) impressive book. Its greatest merit is that it makes a conscious effort to embed the study of the United Nations in international relations and organization theory. In his ambition to recapture for international organization the high ground now held by regime theory, Rochester tends to spread his forces somewhat thin. His analysis ranges from broad macro levels (system transformation, world order) to narrow micro levels (organizational engineering); and he has descriptive, as well as normative, aspirations. The frequent quotes and references to an array of more or less relevant sets of literature underscore the eclectic character of his endeavor.

One limitation of Rochester's analysis is his preoccupation with *formal* organization. It has often been said that the great value of the United Nations has less to do with what goes on in formal sessions than with the fact that the UN headquarters is a unique conglomeration of diplomats from virtually every country in the world, who can meet informally in various constellations without publicity. Similarly, interagency coordination is often facilitated by the emergence of informal networks, whereas formal designations of a specific lead agency (one of Rochester's prescriptions) tend to aggravate organizational jealousies and turf battles.

By training his searchlights on the intergovernmental organizations (IGOs) of the UN system, Rochester leaves nongovernmental organizations (NGOs) in the shade. This also means that he underestimates the importance of IGO–NGO links in international cooperation. To produce action—to implement policies—NGOs are often needed. They are particularly significant when dealing with issues that span international and community levels and where reaching into societies is of utmost importance. (Population and AIDS are examples in point.) Moreover, NGOs are not as easily paralyzed by claims to state sovereignty as are IGOs. Unfortunately, IGO–NGO links are mostly weak, and much remains to be done to overcome the gap of mutual distrust separating IGOs and NGOs in various issue areas. To establish arenas and channels for IGO–NGO coordination should therefore be part of the reform agenda.

In sum, *Waiting for the Millennium* is a useful antidote to the atheoretical, descriptive works on the United Nations whose ranks will no doubt swell further as the organization approaches its fiftieth anniversary. With its rare combination of theoretical anchoring and operative proposals, this book will be of interest to scholars, as well as practitioners, of international organization.

University of Lund CHRISTER JÖNSSON

The Elusive Transformation: Science, Technology, and the Evolution of International Politics. By Eugene B. Skolnikoff. Princeton: Princeton University Press, 1993. 322p. $39.50.

This book offers a very well written and thoughtful overview of the likely impacts of science and technology on international politics. Written as an extensive personal reflection by someone who has studied these issues for more than three decades, it bounces off of—and avoids—many of the more glib and startling predictions that have been made about such impact, concluding that science will probably not induce revolutionary changes in the international system of separate national sovereignties.

Skolnikoff surveys just about all the possible areas normally collected into "science policy," including broad issues of arms control and information management and, more generally, international economics. It includes discussions of demographic issues and of the possibilities of ecological damage, including, in particular, global warming. While not written as a text, the book would serve admirably for courses on science and public policy as an antidote to the exaggeration and hyperbole one often finds in this field. The author definitely sounds a warning that current political arrangements may be deficient in handling the complexities of rapidly evolving technologies; but at the same time, he stops short of concluding that such political institutions will respond by accepting major changes.

The book is very rich in citations of other literature and of relevant aggregate data. In its discussions of domestic economics and international economics, it wanders, at points, beyond what one might have regarded as science and technology policy more narrowly and precisely defined. The author endorses the general proposition that the pace of scientific development is much more rapid than in the past, thus confronting us with very different phenomena and problems. Yet the coverage is again a little too broad to fit entirely with this characterization. For example, Skolnikoff cites the AIDS epidemic as one problem that might, indeed, require more revolutionary responses but at the same times notes the analogy to the plague and other epidemics that hit the world a long time ago. Similarly cited is the possibility of an asteroid's striking the earth, which, again, may have happened with dramatic consequences in the past (though long ago).

The authors' sweep is thus broad enough to include somewhat more than what is new and different for the late twentieth century. Yet his general conclusion still seems very thoughtful and well grounded, namely, that there have been too many predictions by which what is indeed new and different about our science and technology will require—and will cause—very new and different political arrangements.

Skolnikoff presents a very issue-driven analysis of politics—as science and technology are the independent variables, and politics the dependent. A reader looking for extended discussions of the internal processes of government agencies handling these issues may thus be slightly disappointed. The book offers some wise observations about the roles and limitations of intergovernmental and nongovernmental organizations and of the future of collective security, set against the continuing debate between "liberal" and "realist" analyses of international relations. It is very sensible and clear-headed in its discussion of military issues and arms control.

The most salient conclusions that Skolnikoff offers are not so very startling, namely, that command economies are doomed in being the least able to handle the complexities of modern science and technology and that the flow of information across boundaries will make it increasingly difficult for *all* governments to constrict such information. Skolnikoff advocates, (but not with excessive vehemence) policies of "targeted research and development."

Yet the overall thrust of the book is, indeed, that the most startling conclusions are likely to be wrong here and that this work, indeed, catalogs a great number of such conclusions that seem to be more wrong than right.

University of Maryland GEORGE H. QUESTER

Coping with Complexity in the International System. Edited by Jack Snyder and Robert Jervis. Boulder: Westview, 1993. 366p. $60.50.

In the 1960s, academic students of international relations frequently spoke and wrote about "the international system" and "systems theory." In the reader that virtually defined the field at the end of that decade, James Rosenau, declared that "of all the advances that have occurred in the study of international phenomena, perhaps none is more important than the ever-growing tendency to regard the world as *the international system*" (*International Politics and Foreign Policy* [1969], p. 71; original emphasis). Students of world politics became familiar with concepts such as system dominance, feedback, and equilibrium.

Although the language of systems became a conventional way of describing international politics, none of this work led to general explanatory theory. Indeed, during the 1970s, behavioral systems theory became unfashionable, overshadowed by Kenneth N. Waltz's powerfully argued reformulation of realist theory (*Theory of International Politics*, 1979). Discussions of general systems theory and cybernetic approaches to international politics were replaced on graduate reading lists with arguments for and against Waltz's neorealism.

For the last decade or so, increasing discontent has been expressed with neorealist theory. Elegant though it surely is, it seems to explain little of what puzzles us about world politics—both because strategic interaction among small numbers of actors has a high inherent degree of indeterminacy and because neorealism omits so much of what is significant in world politics (e.g., variation in information, institutions, patterns of economic interdependence, and domestic politics). That neorealism is "too parsimonious" and underspecified has become a constant refrain in the field.

Coping with Complexity adds the voices of 13 authors—mostly associated with Columbia University—to that chorus. (Unfortunately, the volume contains no information about their affiliations or other accomplishments, except for the editors, who are well known.) Jack Snyder's introduction writes of "new thinking" about the international system, which sounds remarkably like the thinking of the 1960s: the world is complex, and many variables matter. Snyder does make an interesting point about complexity: Waltz's system is one of "disorganized complexity" with a small number of actors, but the complexity of world politics is actually largely "organized complexity," because states are "part of a hierarchical, organized system when facing their own societies" (p. 11). In the essay that provides the theme for the volume, Robert Jervis emphasizes the frequency with which action in world politics, as in other systems, has unintended consequences and provides numerous fascinating examples to support this undoubtedly correct proposition. Thomas Christensen, in the conclusion to the volume, uses some of the contributions in it to comment once again (as Eurocentric, historically oriented political scientists of the 1960s did) on the failure of such policies as those of the allies toward Germany after World War I. Except for the references to Waltz's theory, all of these ideas would have been familiar to students in international-politics seminars during the early 1960s.

Although this book hardly puts forward a theory, its contributions all purport to be informed by the *systems perspective* or what Paul Schroeder describes as "the general theme of this book, linear versus systemic thinking in international politics" (p. 48). The thread connecting the chapters is provided by Robert Jervis's essay, which focuses on the "perverse effects" of actions within a systemic context. Jervis does not provide a systematic statement of what he means by *systems effects*; but from his discussion and examples, it seems clear that he wishes to emphasize unintended and unwanted consequences resulting particularly from strategic adjustments. For instance, a state's purposive behavior may be thwarted by reallocation of others' resources or by the formation of new coalitions; that is, Jervis is concerned with the ways in which systems can produce *countervailing actions*. Unlike linear thinking, systemic thinking anticipates these countervailing actions and leads to more appropriate behavior.

Unfortunately for the coherence of this volume, its contributors have several different conceptions of what constitutes systemic thinking or, more broadly, systemic effects. A variety of interaction effects nestle beneath the systemic umbrella. Schroeder's fascinating essay on eighteenth- and nineteenth-century diplomacy does follow Jervis's theme; Kathleen McNamara's discussion of the European Community's Common Agricultural Program emphasizes unintended effects; and Randall Schweller's chapter on Hitler's strategy focuses on coalitional dynamics. But Jonathan Mercer's essay defends linear thinking against too great a preoccupation with reputation, and Susan Peterson focuses on bilateral diplomacy between Britain and Russia before the Crimean War; her systemic effects refer to links between international and domestic politics, not countervailing strategic action. William McNeil focuses on the *cognitive* complexity of the war debt–reparations–loans–domestic politics system of the 1920s, which be believes Weimar's leaders failed to understand.

William Daugherty, Jeffry Frieden, and McNamara describe self-reinforcing systems that display the *opposite* property of that discussed by Jervis: reinforcing spirals of cooperation. (The Concert of Europe, the classical gold standard, and the Single European Act are the cases in point.) On the other hand, Richard Betts—in an updating of the critiques of collective security a generation ago by Inis L. Claude and Arnold Wolfers—warns against easy expectations that maintenance of collective security under benign conditions will contribute to success when real threats develop. Anne Uchitel's chapter misinterprets Keohane and Nye's *Power and Interdependence* (1977) as claiming that interdependence enhances systemic stability and, in proceeding to argue that dependence can create instability, entirely ignores the theory of lateral pressure of Nazli Choucri and Robert C. North (*Nations in Conflict*, 1975).

Clearly all of the dynamics discussed in this book are systemic in some sense (they depend on interaction effects that would not be present were only two unitary actors involved), but to recognize this similarity is to suggest how uninformative a mere focus on systems is. If it is a platitude that world politics constitutes a system, we should hardly be surprised when a book resting on this recognition does not yield new insights. There are worthy contributions in this volume (the chapters by Schroeder, Mercer, Frieden, Betts, and McNamara are particularly interesting); but the editors' framework, with its systems approach, is not sufficiently precise to give the book much unity. As Jervis and other contributors admit, the systems approach is more an approach than a theory; it does not yield clear hypotheses, although decision makers' awareness of systems dynamics may "reduce the incidence of perverse effects somewhat" (p. 43). *Coping with Complexity* is more valuable as a reminder of what we have long known but are perhaps prone to forget than it is as an innovative contribution to theory and research.

Harvard University ROBERT O. KEOHANE

The Elusive Balance: Power and Perceptions during the Cold War. By William Curti Wohlforth. Ithaca: Cornell University Press, 1993. 317p. $39.50 cloth, $16.95 paper.

Wohlforth argues that general international-relations theory has failed to progress because of ambiguities in the central concept of power. It is difficult, for example, to test the hypothesis that the system automatically returns to equilibrium when international-relations specialists make contradictory estimates of the distribution of power for the same time period. Even quantitative measures of power contain so many disparate elements such as military, technical, economic, population, and national resources that the analyst can achieve practically any desired result merely by selecting the correct indicators. Wohlforth comes heretically close to suggesting that there is no objective underlying distribution of power—hence his title. Even a major war does not reveal states' relative capabilities, because wars are often ended by negotiation; and the relative influence of particular states on the postwar settlement may have more to do with expectations about the postwar hierarchy of prestige than about reality.

He reasons that if power influences international politics, it must do so mainly through the perceptions of people who make decisions on behalf of states. For each major historical era between 1945 and 1989, Wohlforth discusses Soviet views of the elements of power, the distribution of power, the mechanics of the balance of power, and the hierarchy of prestige. He also examines United States perceptions, though rather superficially.

The book provides a very useful and lucid synthesis of the many arcane yet consequential debates among Soviet ideologists and theorists over international relations. It should become a standard reference source for historians and political scientists, because it brings together the insights of many different Kremlinologists, shows where they disagree, and presents the most recent consensus of opinion.

Wohlforth is also more tangentially interested in evaluating various explanations of Soviet foreign policy learning. How could the Soviets pursue a set of foreign policy ideas that isolated them from the rest of the world and directed them to seek additional territory and influence, at the cost of provoking a hostile balancing coalition? He presents a fascinating account of the incubation of *perestroika*, making a good case that Soviet learning was not inevitable. New ideas about foreign relations were available from academics from 1980 to 1985, but among the Soviet political elites, only Mikhail Gorbachev was listening.

Experts on the former Soviet Union are engaged in a debate, going back to Barrington Moore and George F. Kennan, over whether Soviet foreign policy was primarily motivated by *external* influences, such as a hostile international environment, or by *internal* causes, such as factional maneuvering within the Kremlin or the need to build a coalition among various elite groups. Wohlforth presents a more sophisticated interactionist view, whereby foreign policy beliefs are a response to environmental challenges and opportunities but may also serve domestic political purposes. That particular ideas were useful to Stalin in his struggle with political opponents does not mean that the Soviet leader did not believe them. Wohlforth points out that the feedback from their policies was irregular or ambiguous or seemed to confirm erroneous Soviet beliefs, at least in the short term.

In flowing prose, Wohlforth presents many creative and insightful interpretations of Soviet rhetoric and the disjuncture between self-presentation and reality. Particularly interesting is his account of how Gorbachev coped with the decline of the Soviet empire and the collapse of its superpower status by trading in Soviet military chips to achieve arms control agreements, stressing global interdependence and the irrelevance of military power, and by claiming that world stability required a strong, democratic, united Soviet Union.

Wohlforth does not always give sufficient weight to the possibility that the Soviets used public estimates of U.S.–Soviet power relationships for strategic purposes. When the Soviets were vulnerable to a U.S. nuclear strike in the Stalin and Khrushchev eras, how likely is it that Soviet leaders would publicly admit their military inferiority to the United States, particularly when the Eisenhower administration maintained a declaratory policy of responding to communist aggression with "massive retaliation?"

He is too inclined to accept the realist view of the Cold War as a power struggle between the challenger Soviet Union and the dominant United States. There is little reliable evidence, for example, that Stalin was dissatisfied with the postwar settlement or felt that Soviet influence on diplomatic arrangements was incommensurate with their contribution to winning the war against Germany. Stalin achieved just about everything he wanted in Europe. Confronted with the fait accomplis of the Soviet Army, Winston Churchill and Harry Truman found it difficult to oppose Soviet territorial objectives, absorption of the Baltic states, and demands for "friendly governments."

Rather than seeking prestige commensurate with their military power, the Soviets often took apparently belligerent actions in response to such threatening Western actions as the establishment of a West German state, the creation of a peacetime military alliance, and the rearmament of Germany. By focusing solely on perceptions of power, rather than state interactions, Wohlforth overlooks the role played by mutual mistrust, the security dilemma, and conflict spirals in exacerbating Cold War tensions.

He reveals many anomalies in the balance of power that are either ignored or deliberately slighted by hard-nosed realists. For example, states do not always balance against would-be challengers. The Soviets' experience in the 1950s and 1960s supported the correlation-of-forces model, which held that overwhelming military power would compel capitalist states to seek a détente. More controversially, Wohlforth concludes that there is frequently a disjuncture between leaders' perceptions of relative state power and material reality in the form of measurable resources.

Neorealists will not be convinced by his analysis, because many argue that the objective distribution of power constrains and channels the behavior of states regardless of the beliefs and perceptions of national leaders. For example, Stalin may have depreciated the importance of nuclear weapons, but he refrained from interfering with the U.S. airlift and discontinued the Berlin blockade despite his failure to block the establishment of a West German state. Similarly, for all his boasts about Soviet missile capabilities, Khrushchev did not carry out his ultimata in two Berlin crises. The debate over the extent to which power is an objective constraint or a manipulable image continues.

University of California, Los Angeles DEBORAH WELCH LARSON

Engaging the Enemy: Organization Theory and Soviet Military Innovation, 1955–1991. By Kimberly Marten Zisk. Princeton: Princeton University Press, 1993. 286p. $39.50.

Kimberly Zisk examines the conditions under which military organizations are likely to change military doctrines. She argues that "the timing and content of military doctrine innovations are a result of the interaction of international and domestic political factors, of foreign doctrine changes and of domestic civilians' political strategies" (p. 5). The argument is complex and is summarized in its hypotheses: military organizations "develop innovative doctrines on their own, in the absence of civilian intervention," in response to foreign doctrinal shifts that threaten their war plans; before reacting to these foreign threats, however, military officers attend to "domestic threats to their organizational health and autonomy first"; military officers are likely to offer innovations that augment military resources and autonomy, yet these innovations provoke debate within the expert policy community and the military only slowly institutionalizes them; changes in the composition of the policy community can change the nature of the doctrinal innovations (pp. 26–28).

Zisk presents a well-crafted project that draws on a rich theoretical tradition in order to derive hypotheses and then test these in cases that permit controlled comparison. The book's three case studies examine Soviet responses to the United States' doctrinal innovations, including reactions to flexible response, the Schlesinger doctrine, and air–land battle (or follow-on forces attack). The richest and most rewarding of these studies is the last: Zisk demonstrates how public discussion of national security issues changed as civilian analysts from the Academy of Sciences' institutes joined the policy community. She also illustrates the ingenious strategies used by these analysts to develop expertise and build bridges with professional military officers, despite the resistance of some in the military.

Zisk's work is part of the effort to apply social science theory to the analysis of military doctrines—an effort that began anew in 1984 with publication of Barry Posen's *Sources of Military Doctrine* and Jack Snyder's *The Ideology of the Offensive*. Zisk's foil is the proposition, inspired by Graham Allison's work on bureaucratic politics, that military organizations are conservative bureaucracies limited by their standard operating procedures and likely to innovate only in the face of defeat on the battlefield or under pressure from civilians (p. 3). She contends that an improvement over the bureaucratic politics model for understanding the military role in doctrinal change is the "policy community" model, in which "policy ideas percolate fairly freely" among experts "who interact with each other regularly to exchange ideas" and have access to decision makers (pp. 21–22). Zisk concludes by arguing that her findings support a new theoretical approach that rests on three central propositions: (1) that the influence of institutional and organizational interests on the beliefs or behavior of individuals is complicated by idiosyncratic factors; (2) that both international and domestic politics constrain the behavior of individuals and organizations; and (3) that policy outcomes are determined not only by interest groups but also by policy communities (p. 184).

Zisk's argument might be strengthened if she could give more conceptual and operational precision to her dependent variable, military doctrine. Zisk seems torn between arguing that doctrine is formulated by the state on the basis of information provided by the General Staff and that "doctrine formulation was controlled by the General Staff" (pp. 34, 43). Although the latter view is highly doubtful in the Soviet case, it is necessary for the author's major thesis that the military will introduce innovative doctrines on its own without civilian intervention. In the empirical studies, the author does not always identify either the official act that adopted a new doctrine or the content of that doctrine, so that what should be the most important empirical finding in each case study is often hidden in a larger discussion of a debate among military professionals. For example, the author asserts that in 1965 Soviet military doctrine "apparently shifted," that by the mid-1970s it "appears" to have shifted again, and that in the early 1980s there were statements "probably indicating that some sort of doctrinal decision had been made"; but the evidence of official doctrinal pronouncements is not presented. Instead, the author appears to let the debate among military professionals and *institutchiki* stand as evidence of innovation in official doctrine.

Zisk's argument might be further strengthened if she could substantiate empirically or specify theoretically the causal link between doctrinal developments in the United States and the innovations by Soviet military officers. She does not give us standards by which we can judge whether Soviet doctrinal developments were, in fact, responses to American ideas, rather than the autonomous development of autochthonous doctrines. Zisk offers circumstantial evidence that the Soviet debates took place around the time of an American doctrinal shift (although some Soviet debates actually began before the American shift) and that some Soviet military writers, whether part of these debates or not, had taken note of the American shift. The case for a causal link might be strengthened by hypotheses that predict the type of response we should have expected from the Soviets (e.g., symmetrical vs. asymmetrical responses, compensatory vs. exploitative responses). Drawing on the work of Roman Kolkowicz (1970) and James McConnell (1985), such hypotheses could provide theoretical reasons for believing that observed doctrinal developments in the Soviet Union were reactions to U.S. innovations.

Zisk's work might be strengthened by some further attention to the larger domestic political arena in which these military debates took place. Throughout this study, the important invisible hand (invisible from the perspective of the hypotheses) is the Communist party leadership that made doctrine based on the technical information and military science provided by the General Staff (p. 34). It was these leaders who imposed doctrinal innovations in 1960 and in the early 1980s and then "snatch[ed] the reins of doctrinal initiation away from the Soviet military" in 1987 (p. 163). It was the politicians who decided to empower civilian analysts in the late 1980s and thus expand the policy community (p. 23). The hypotheses of this study might ask why the Communist party leadership adopted innovative military doctrines. Indeed, this work raises the question whether in societies with traditions of strong civilian control over the military, one can study doctrinal innovation by limiting hypotheses to the behavior of professional military officers or even policy communities.

These opportunities to strengthen or build on this project should not detract from the significant contribution that it makes to an important debate. Zisk's work reflects the maturing of our field of Soviet and post-Soviet studies, as scholars not only ask how the theories of political science's mainstream might sharpen area studies but also how insights from our case might advance theory. Zisk's work should be taken seriously both by those who seek keener understanding of Soviet military debates in its last three decades and by those who seek to sharpen the organization theory of military behavior.

University of California, San Diego PHILIP G. ROEDER

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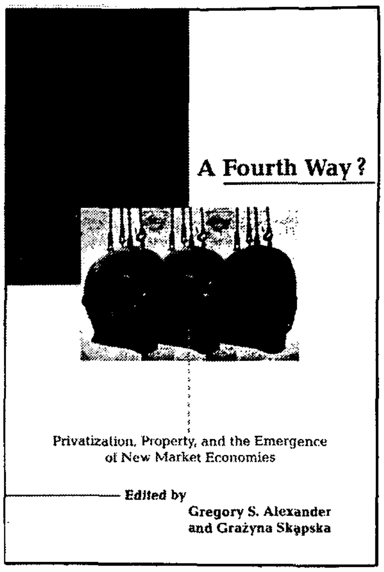

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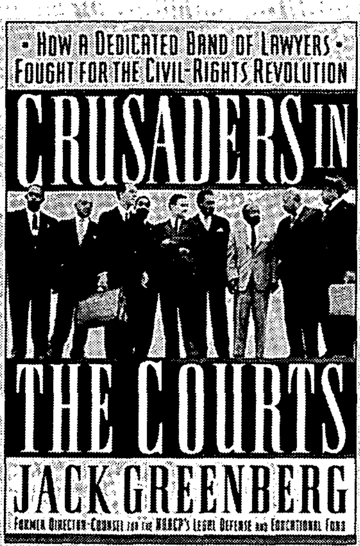
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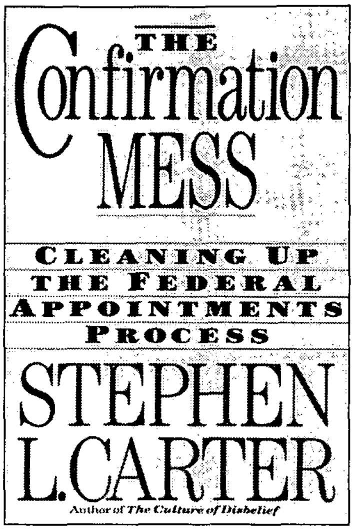
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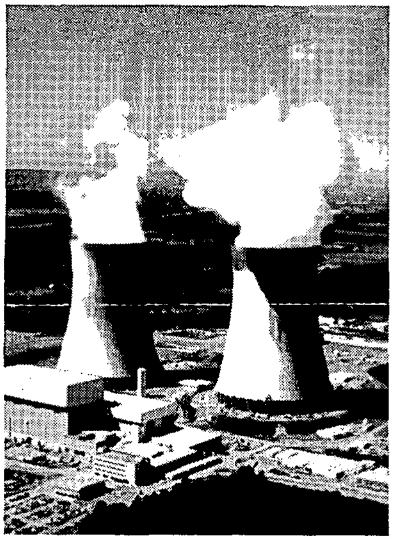

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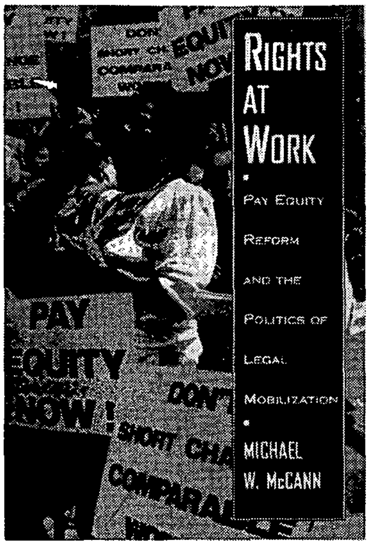

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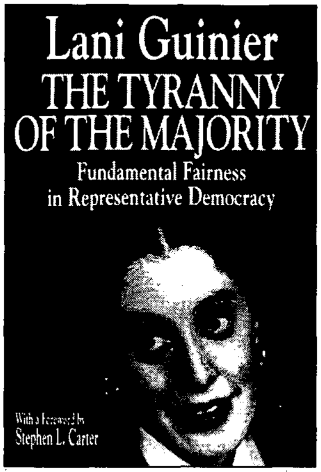
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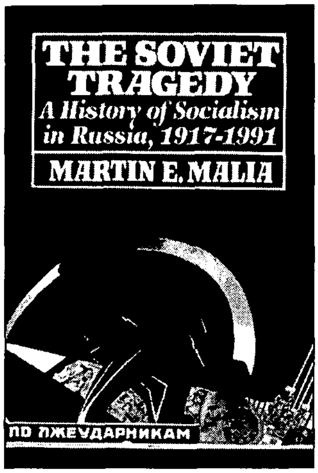

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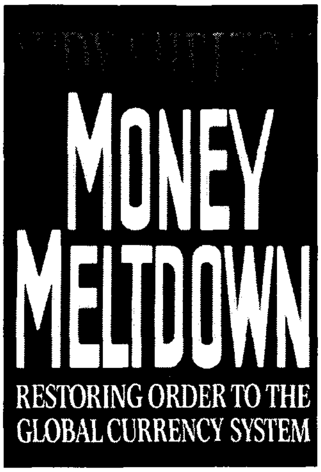

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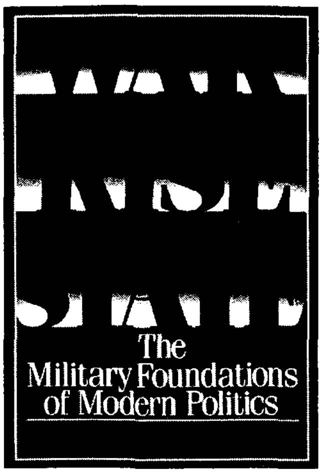

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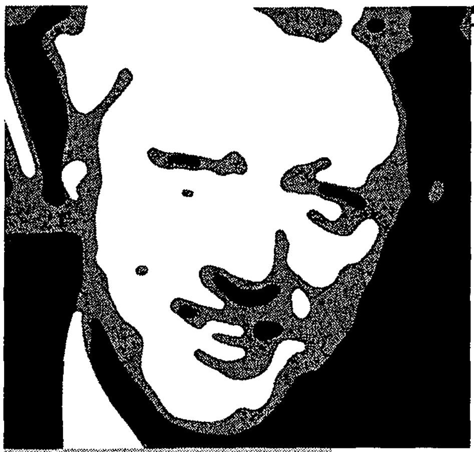

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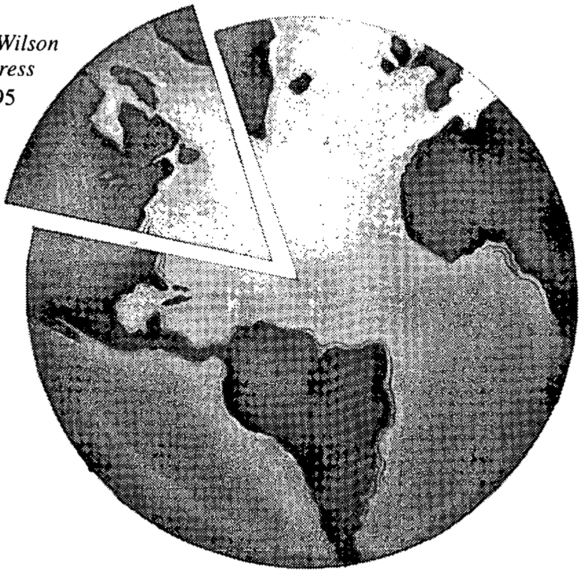

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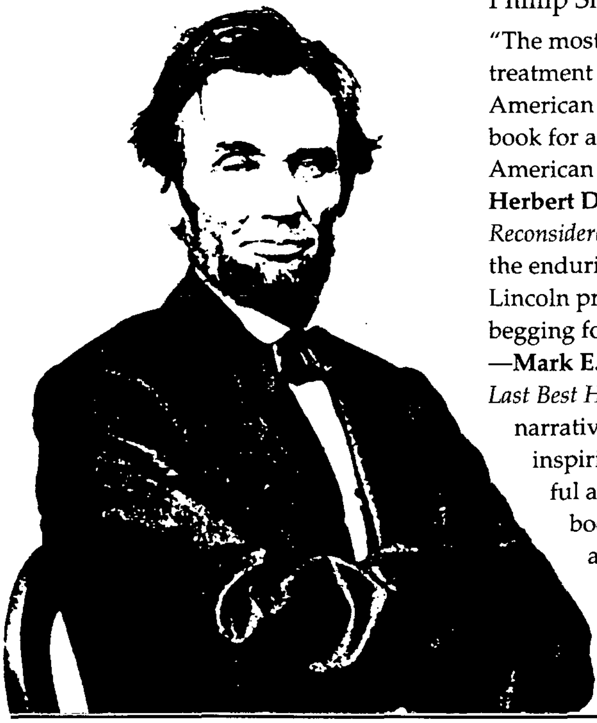

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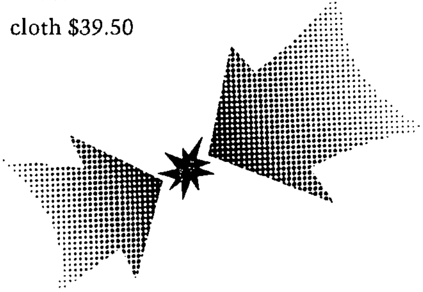

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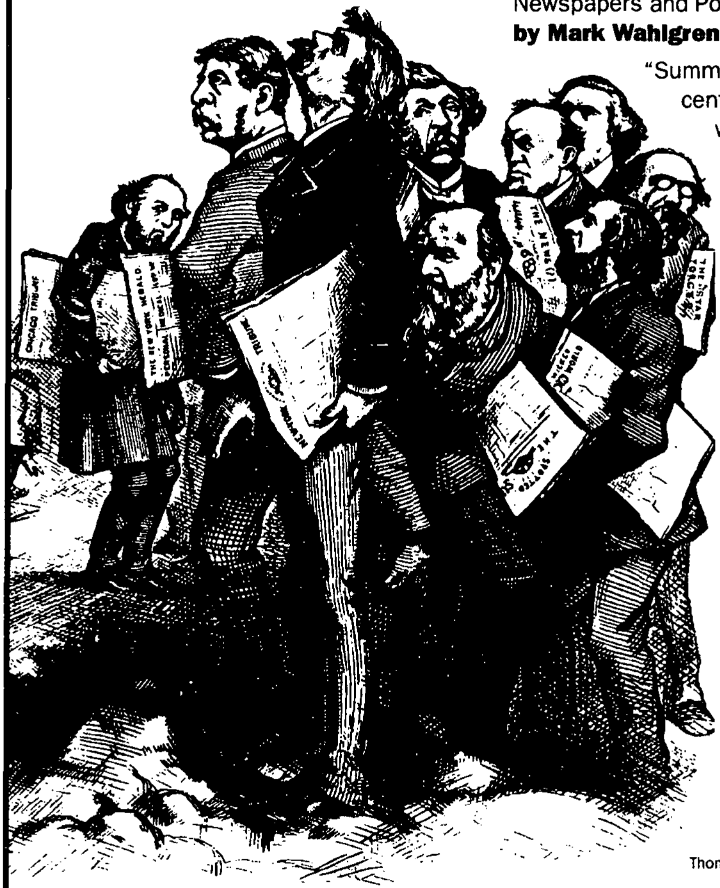

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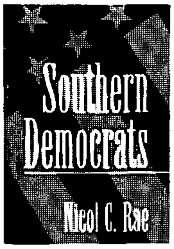

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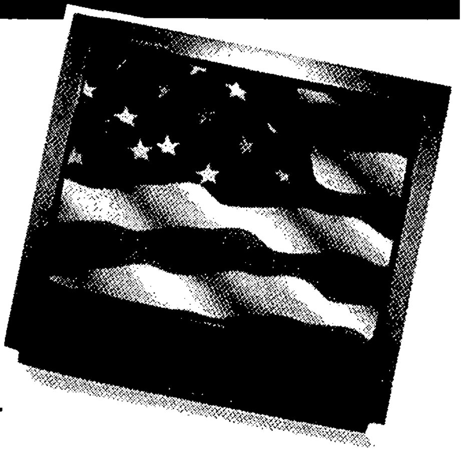

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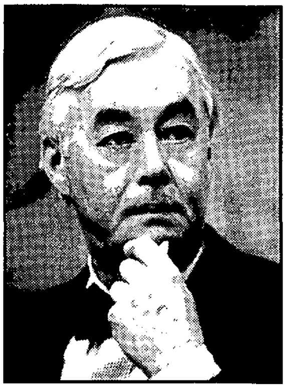

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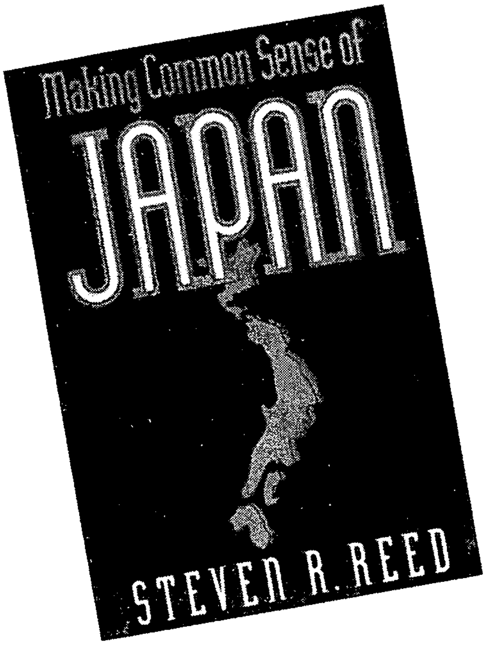

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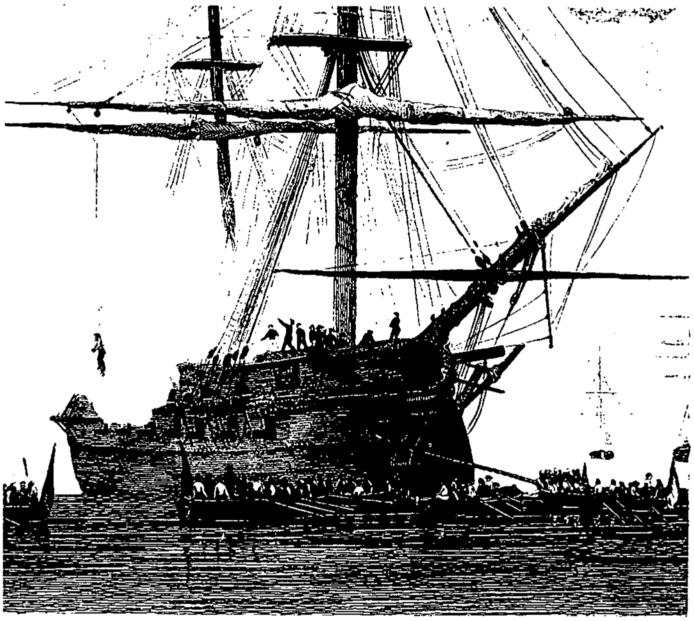

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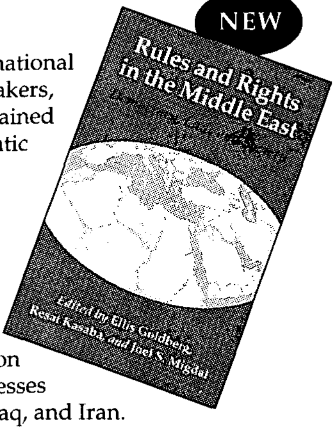
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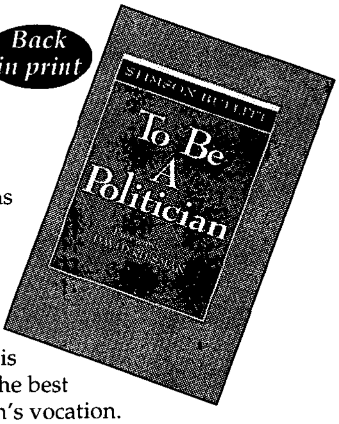

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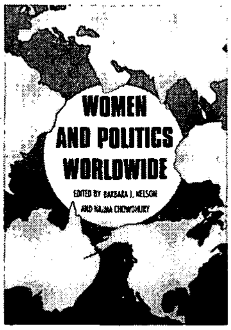
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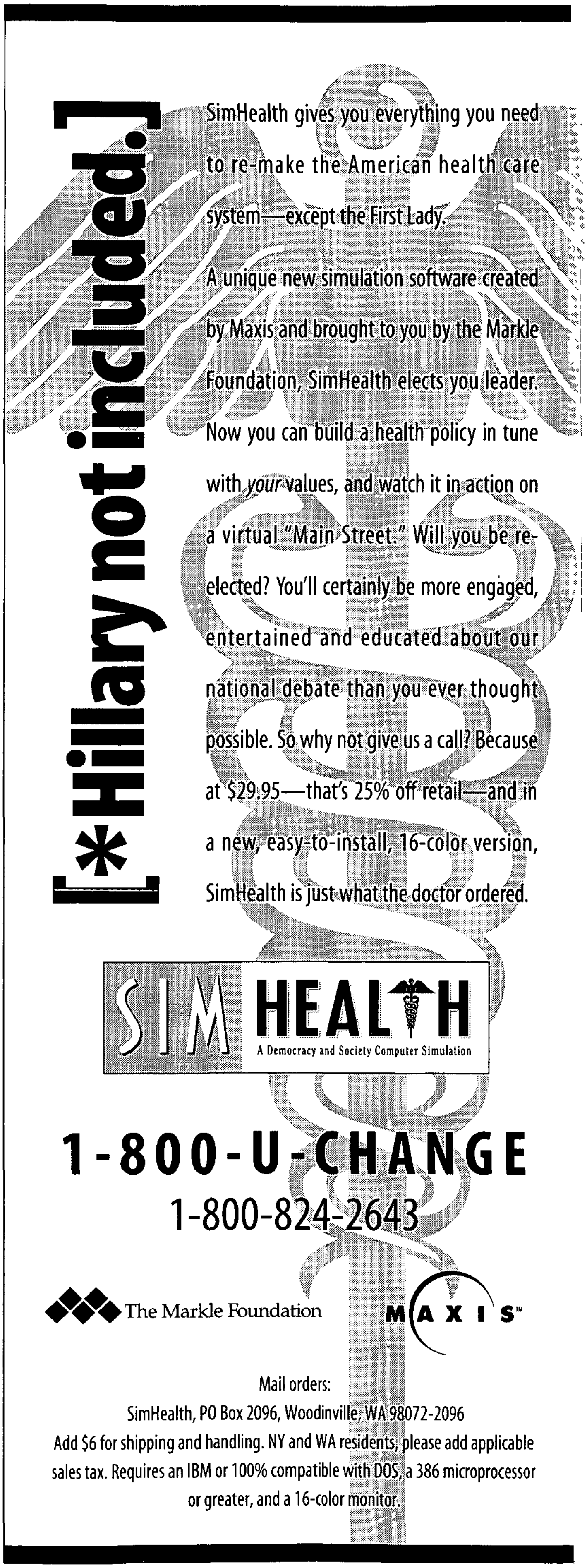

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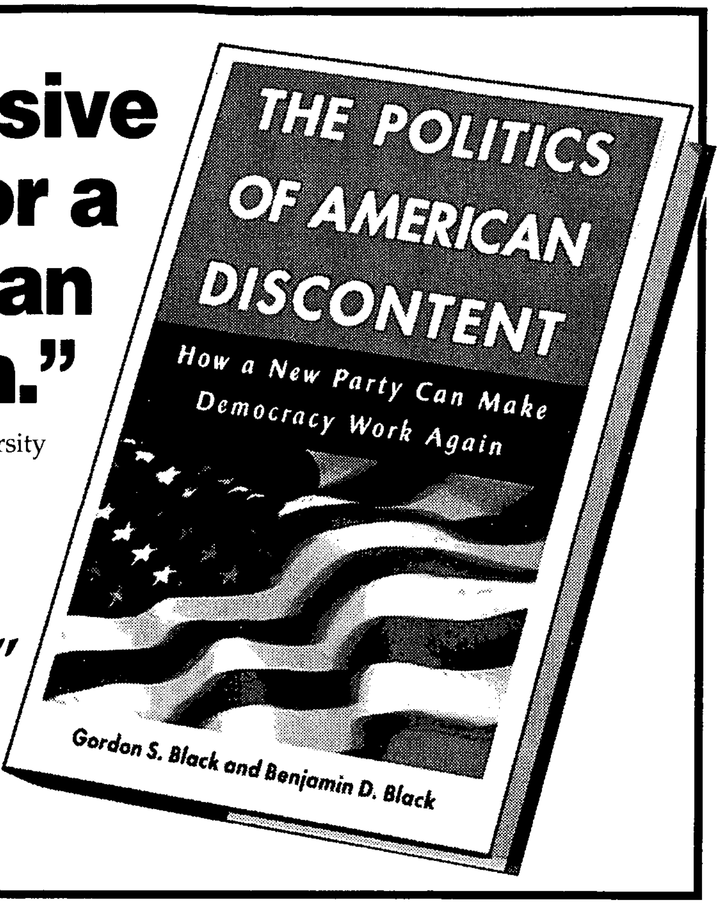
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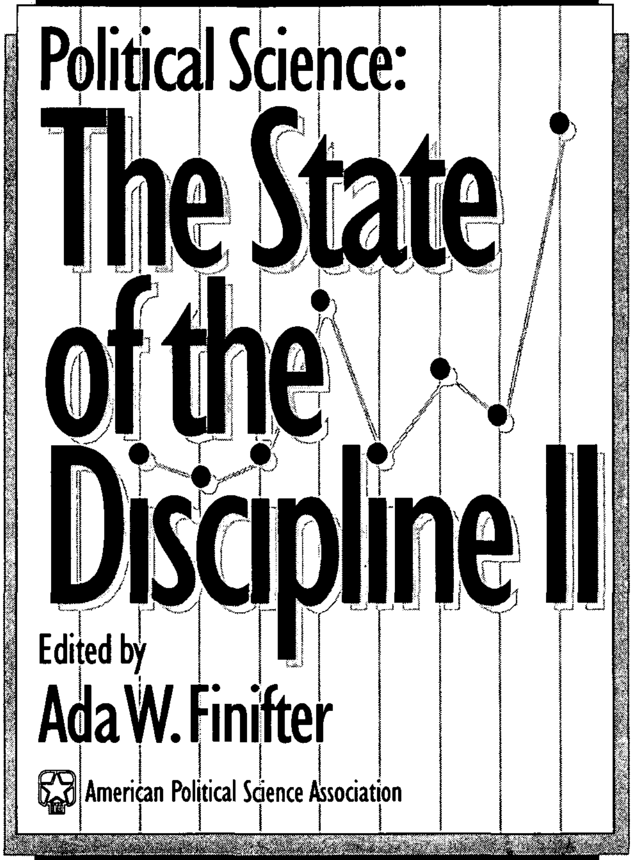
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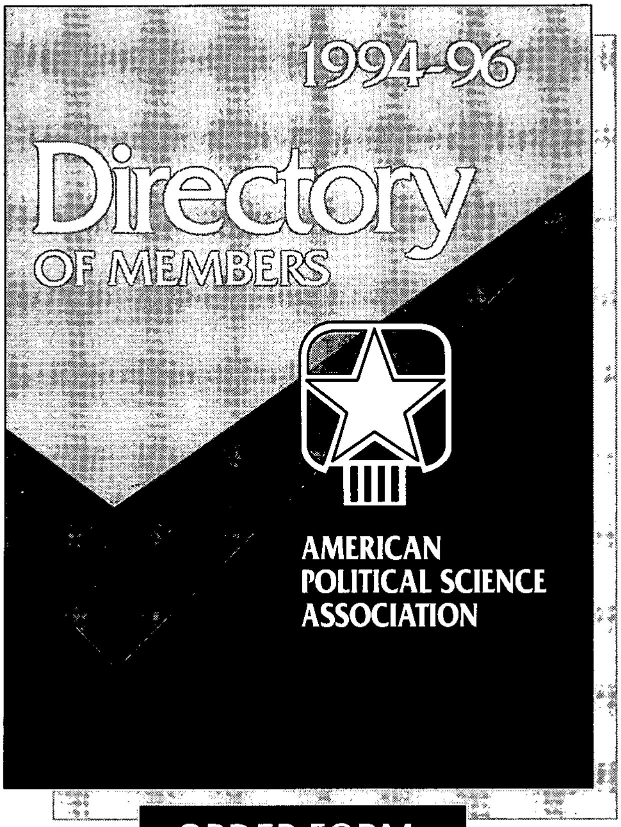

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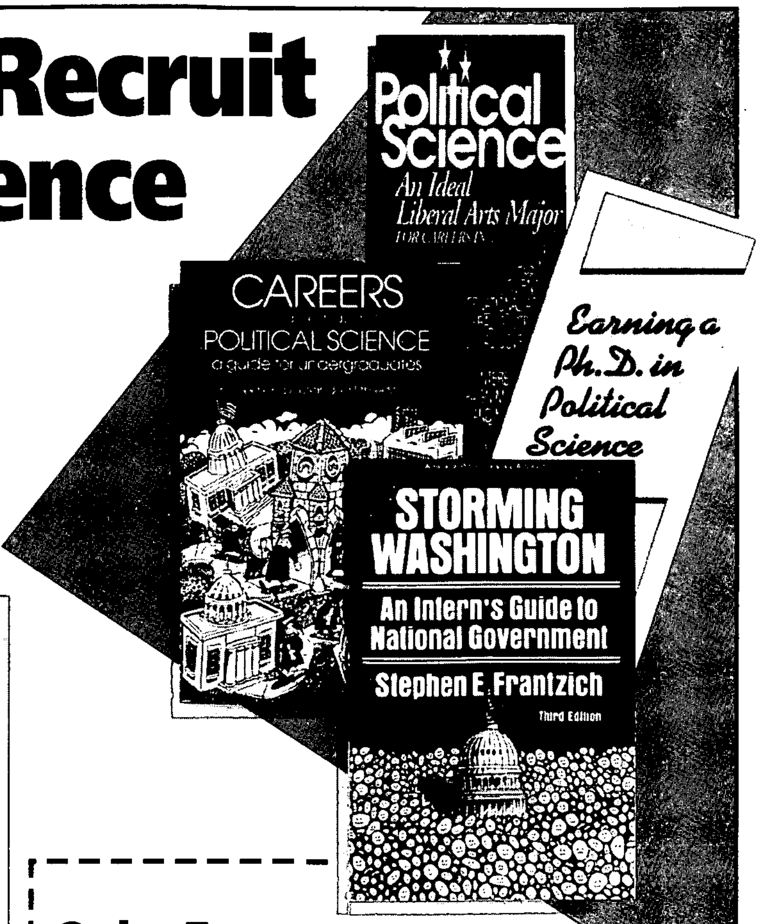
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WHO MAKES PUBLIC POLICY?

The Struggle for Control between Congress and the Executive

Edited by **Robert S. Gilmour,** University of Connecticut, and **Alexis A. Halley,** Meridian International Institute

The question raised by use of an explicit legislative mandate technique rather than the more common informal intervention techniques is whether the more heavy-handed legislative technique produces results closer to congressional intent than less formal techniques.

Diana Evans, Trinity University

Each element of the Nuclear Waste Policy Act raised new issues of significant concern to Congress, state and local governments, and citizens. As constituent pressure against the siting of the nuclear waste sites increased, so did congressional oversight and the number of committees and subcommittees having a stake in radioactive waste management.

James A. Thurber, American University

For the Prospective Payment System for Hospitals under Medicare, Gramm-Rudman and the accompanying reconciliation process institutionalized the relationship between the branches as a game of who gets credit for savings. In essence, the game is that if the administration proposes a change through regulation, it will get credit for a saving; if Congress proposes a change through reconciliation, it will get credit.

Alexis A. Halley, Meridian International Institute

Congressional involvement in the AMRAAM program drew wider attention to cost and technical problems with the system and raised the visibility of the system both in the Air Force and in the DOD generally. Congressional attention, expressed in a variety of intervention tactics, threatened AMRAAM as an Air Force program. In turn this threat stimulated the service and the DOD to rally in support of the program managerially and technically as well as politically.

Robert S. Gilmour, University of Connecticut, and
Eric Minkoff, Department of Justice

The historical pattern of reform in the realm of military command structure generally involved congressional efforts to dilute ambitious executive branch proposals to consolidate power over the military services. In the 1980s, by contrast, Congress pushed for such a consolidation against the wishes of the secretary of defense. Indeed, tension in this period of defense reform was less between the executive and legislative branches than between the Defense Department, on the one hand, and Congress and the White House, on the other.

Thomas L. McNaugher, Brookings Institution, with
Roger L. Sperry, National Academy of Public Administration

There is a compelling reason that Congress is always tempted to meddle in the details of administration. Implementation is—broadly across all of the U.S. government—the primary point of conflict resolution, and thus the place where many important decisions actually get made. Congress seeks a role in implementation because that, so often, is where the action is.

G. Calvin Mackenzie, Colby College

These cases show a "congressional co-manager" intervening directly in the details of policy development and management rather than enacting vague, wide-ranging, sweeping statutes to change fundamental policy directions. They also suggest that congressional co-management is as much a result of actions in the executive branch as it is a result of actions in the legislative branch.

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